THE EFFECT OF IRS ENFORCEMENT POLICIES ON TAXPAYER ATTITUDES AND COMPLIANCE: A PROCEDURAL JUSTICE APPROACH

By

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A DISSERTATION PRESENTED TO THE GRADUATE SCHOOL
OF THE UNIVERSITY OF FLORIDA IN PARTIAL FULFILLMENT OF THE
REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY

UNIVERSITY OF FLORIDA

1994

ACKNOWLEGEMENTS

I would like to thank the members of my committee, Dr. Sandy Kramer, Dr. Gary McGill, Dr. Robert Knechel, and Dr. John Lynch for the time and energy they put in to help me complete this project. I especially want to thank my chairperson, Sandy Kramer, for helping me to complete this dissertation on schedule and for the guidance and encouragement she gave me during my entire program at the University of Florida. I will always be grateful for her efforts on my behalf.

Also, I would like to thank the Ernst & Young Foundation. The funds I received from their dissertation grant made it possible for me to dedicate more of my time to completing my dissertation during this past year.

In addition, I want to acknowledge the contribution of the community groups and civic organizations that provided subjects for this study. Their willingness to help with recruiting subjects helped tremendously.

Finally, I want to thank my wife, Anne, and my children, Matthew, Daniel, and Whitney, for their support and for graciously enduring the hardships that we faced during my doctoral program.

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Abstract of Dissertation Presented to the Graduate School
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April 1994

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It has been suggested that tax collection procedures that are perceived to be unfair may, in some instances, actually reduce the level of tax compliance. The purpose of this study was to determine if tax collection procedures that are perceived to be unfair cause taxpayers to be less supportive of compliance with the tax laws and to actually underreport taxable income.

This study draws upon procedural justice theory to predict how taxpayers will respond to what they perceive to be unfair treatment by tax authorities. First, it was hypothesized that taxpayers subject to inconsistent tax enforcement procedures would more strongly agree that noncompliance is acceptable and underreport more of their taxable income. Second, like inconsistent enforcement procedures, tax enforcement procedures based upon inaccurate information were predicted to affect taxpayers' attitudes and behavior negatively. Finally, it was hypothesized that unfair tax enforcement procedures experienced vicariously would adversely impact taxpayer's attitudes and behavior.

These hypotheses were tested using an experiment. Subjects, who were experienced taxpayers, participated in a personal financial decision-making simulation where the true purpose of the experiment was disguised. While participating in the simulation, subjects earned cash awards for civic organizations based upon their decisions during the experiment.

Of the three dimensions of procedural justice tested, only one produced the predicted effect on behavior. The research results showed that subjects penalized because the tax enforcement authority in the experiment used inaccurate information did not respond by underreporting more income. Moreover, subjects exposed to inconsistent tax enforcement procedures also did not underreport more income. However, subjects exposed to procedural injustice indirectly by learning of another's unfair experience did respond by underreporting more income. Finally, none of the manipulations of procedural justice in the experiment produced the predicted effects on taxpayer attitudes.

CHAPTER 1
INTRODUCTION AND BACKGROUND

Introduction

Noncompliance with the tax laws is a serious problem in the United States. Estimates place the tax gap for 1986 at $70 billion dollars, which was approximately 40% of the federal budget deficit at that time (Internal Revenue Service 1988). The traditional approach to dealing with this problem--the use of audits, penalties, and third-party reporting--has not been effective in reducing this gap. For example, Internal Revenue Service estimates show that all of its enforcement programs combined would reduce individual noncompliance by only 31% (Internal Revenue Service 1990).

Lewis (1982) suggests that there are two approaches to improving tax compliance. The first involves an increase in fines and audits. However, the viability of this approach is limited due to IRS budgetary constraints. The second approach, which Lewis suggests would be more productive, is to improve attitudes towards the IRS and the federal government. Recently, the IRS realized that audits and penalties result in limited improvement in tax compliance and that it needs to try alternative techniques for improving tax compliance.[1] Academic research has shown that using a carrot instead of a stick to motivate compliance may indeed be promising. According to this research, appeals to conscience (Schwartz and

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[1] The IRS recently has initiated its Compliance 2000 program, which is aimed at increasing taxpayer satisfaction by becoming more service oriented in its dealings with taxpayers.

Orleans 1967) as well as other positive incentives (Alm et al. 1992b) may improve compliance.

One nontraditional approach the IRS could take to enhance tax compliance would be to increase the fairness of its procedures. Congress has mandated that the IRS do so. In an effort to curb alleged misconduct by the IRS, Congress passed the Taxpayer Bill of Rights as part of the Technical and Miscellaneous Revenue Act of 1988. Among other things, the Taxpayer Bill of Rights entitles taxpayers to a pre-audit written explanation of their rights and of the obligations of the IRS, requires abatement of penalties assessed because of taxpayer reliance on IRS written advice, and allows taxpayers to recover damages in instances where the IRS fails to remove an unlawful lien. However, the congressional subcommittees charged with overseeing the activities of the IRS still believe there continue to be instances when taxpayers are not treated fairly. In fact, both the Senate Finance Committee and the House Ways and Means Committee introduced legislation in 1992 to further limit the power of the IRS.

This attempt to place additional limits on IRS power was brought about by complaints not addressed in the original Taxpayer Bill of Rights. Many of these complaints were related to IRS notices mailed to taxpayers. Specifically, underreporter and nonfiler program notices demanding more tax or penalties were wrong nearly half the time according to a government study (General Accounting Office 1988). Tax practitioners reported in informal surveys that the situation is even more serious. They claimed that these notices are almost always wrong. Even in those instances when the taxpayer is right, these notices demand the time, money, and attention of the taxpayer.

Similar problems extend to other IRS enforcement activities. Taxpayers complain that the IRS has become more aggressive in placing liens and levies upon taxpayer property. A recent study published by the General Accounting Office (1990) supports this claim. During the last ten years, the number of liens filed by the IRS have doubled and the number of levies have quadrupled. Further, this report estimates the IRS made 16,100 seizures of property in 1986 from the wrong people or after the tax bills had been paid. This number represents about 2.3 percent of the total levies initiated during that year. Relative to the number of total levies, the number of erroneous levies may not seem problematic; however, it is these cases that receive attention from the media and thus tarnish the image of the IRS.

Traditionally, complaints about the fairness of the tax system have been directed toward the structure of the tax code. For example, a frequently debated issue is the degree to which the tax code is vertically or horizontally equitable. However, some consider tax fairness to be more encompassing. Senator David Pryor, D-Ark., chairman of the Senate Finance Subcommittee on Private Retirement Plans and Oversight of the Internal Revenue Service, recently expressed a similar view. He commented that "whether the IRS is treating taxpayers, especially middle-income taxpayers, fairly is inseparable from the ongoing debate about the overall fairness of the tax system."[2]

Purpose of the Research

This paper draws upon procedural justice theory to predict and explain how taxpayers respond to unfair treatment during the tax enforcement process. Procedural justice theorists posit that procedural justice is a multidimensional construct that can be broken down into criteria that individuals use to evaluate the fairness of such procedures as those used in tax enforcement (Leventhal 1980). These criteria are consistency, bias suppression, accuracy, correctability, representativeness, and ethicality. If a procedure has been judged to be unfair according to these standards, procedural justice theory predicts that these feelings of procedural injustice will indirectly reduce compliance with laws by reducing the perceived legitimacy of authority (Lind and Tyler 1988).

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[2] Congressman Pryor's (D-Ark.) remarks were made at a Senate Finance Subcommittee hearing on IRS abuses held December 10, 1991.

This research examines the impact of IRS enforcement policies on taxpayer's normative attitudes towards tax compliance and on actual compliance. Specifically, its purpose is to investigate the effects of unfair IRS enforcement procedures--experienced both personally and vicariously--on taxpayer attitudes and compliance. To accomplish this, three separate hypotheses are tested. The first two hypotheses deal with the individual effects of two dimensions of procedural fairness: consistency and accuracy. It is hypothesized that tax enforcement procedures lacking in consistency and accuracy increase a taxpayers' attitudinal propensity to cheat as well as the level of actual cheating behavior. Unlike the first two hypotheses, the third hypothesis tested predicts that unjust tax enforcement experienced vicariously through exposure to another individual's unfair treatment by the tax enforcement authority also will produce the same negative effects on taxpayers' attitudes and behavior.

Motivation for the Research

Previous research dealing with the relationship between procedural justice perceptions and tax compliance has produced mixed results and suffers from methodological limitations

(Colson 1989, Hunter 1987, Kinsey 1992, McGraw 1989, Porcano 1988, Smith 1987, Smith 1992, Song and Yarbrough 1978, Yankelovich, Skelly & White 1984). These limitations exist because most[3] of the studies in this area use survey data to test procedural justice effects on tax compliance. Because survey questions dealing with past or intended tax compliance behavior are sensitive, subjects may not accurately reveal their true behavior. Research comparing self-reports of past tax evasion with actual tax evasion behavior obtained from tax records has shown the correlation between the two is very low suggesting that the validity of admitted tax compliance as a surrogate for real behavior is questionable (Elffers et al. 1987). However, other research suggests that the low correlation between self-reports and audit reports may be due to the different interpretations of noncompliance that taxpayers and auditors use rather than to measurement error (Kinsey 1988). In addition to the measurement problem that may exist when using self-reports, research using survey methods cannot discern the direction of causation when a significant correlation is discovered (Alm 1991). Accordingly, existing research has not established whether procedural justice concerns are used as ex-post rationalizations for noncompliant behavior or whether they cause noncompliant behavior.

Beyond these problems with measurement and establishing causality, the use of surveys in this line of research is limiting because most respondents to the surveys have not had personal contact with the IRS. Moreover, many of the respondents who have had personal contact with the IRS cannot successfully underreport their income because it is subject to third party reporting rules. For these reasons, prior survey research in this area has been unable to obtain a large enough sample of taxpayers who may feel they have personally been mistreated by the IRS and who have the type of income that could be underreported without a high risk of detection. Accordingly, prior research has been unable to make inferences concerning the effects of procedural injustice on this group of individuals. Therefore, the results that these studies report primarily reflect the effects of experience that has been gained by hearing of anothers' problems with the IRS.

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[3] The studies by Colson (1989) and Hunter (1987) use actual IRS data to test for procedural justice effects on tax compliance. The remainder of the studies in this area all employ survey data.

In general, prior researchers in this area have been ad hoc in their approach to measuring procedural justice perceptions. Accordingly, the questions used to measure these perceptions are inconsistent from one study to the next. Further, the questions do not reflect the various dimensions of procedural fairness that are described in the procedural justice literature. Therefore, the measurements of procedural equity perceptions used in the prior research are imprecise and do not allow for a test of the effects of the separate components of procedural justice on tax compliance behavior.

This research extended the prior research in several ways. First, this research was conducted using a laboratory experiment. This made it possible to obtain a measurement of tax compliance that was less likely to be tainted by self-presentation concerns present in surveys. Also, because of the experimental controls present in the laboratory, causation, or the lack thereof, could be unequivocally established. Next, this research drew from procedural justice theory to select and to measure separately the effects of two components of procedural justice-- consistency and accuracy. Last, this research has added to the existing research in that it effectively tested for the direct as well as the indirect effects of procedural justice perceptions resulting from IRS contact.

Overview of Research Method

The effect of procedural injustice on taxpayer's attitudes and on tax reporting behavior was studied using computers to generate an experimental taxpaying setting. To obscure the true purpose of the experiment, subjects were asked to make tax reporting choices in the context of a business and investing simulation over an indefinite number of time periods.[4] In each time period, subjects were asked to make investment decisions and to make tax reporting decisions regarding business and investment income that they had earned in the period. The amount of business income reported in each period was measured to determine how tax reporting choices were affected by the experimental manipulations of procedural justice. In addition, when the seventh period was concluded, subjects answered questions from an attitudinal scale to determine if the experimental manipulations produced a differential affect on subjects attitudes. Subjects earned cash rewards for community groups they represented based on the decisions they made during the experiment.

Overview of Research Results

One of the three null hypotheses dealing with the effects of procedural justice on behavior were rejected and two were not. Subjects exposed to a procedural injustice indirectly by learning of another's experience responded by underreporting more income. The significance of this result, however, was dependent on the approach used to measure tax reporting behavior. Also, it was dependent upon the statistical methodology used to assess the relationship between indirect inequity and taxpaying behavior.

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[4] In actuality, there were seven time periods during the experiment. However, subjects were not told home many time periods they would experience.

Initially, subjects who were penalized because the tax enforcement authority used inaccurate information appeared to underreport more income in absolute dollar amounts and more frequently than those who were not penalized. However, this effect was due to intial differences in the treatment and control groups.

Subjects exposed to an inconsistent audit rate that they believed to be higher than the rate for other subjects--a violation of the consistency criterion of procedural justice--did not underreport more income. In fact, the opposite result occurred. They reported a significantly greater amount of income than those not exposed to the inconsistent audit rates. Finally, the manipulations of procedural justice in the experiment failed to produce the predicted effects on taxpayer attitudes.

Organization of Remaining Chapters

Chapter 2 contains a review of the tax literature related to this research. In Chapter 2, the economic model of tax compliance, prior research dealing with the effects of taxpayer's distributive and procedural fairness judgments on tax compliance, and prior research on the effects of IRS contact on tax compliance are all examined. A discussion of procedural justice theory, which underlies this research, and a discussion related to the development of the hypotheses tested is found in Chapter 3. Chapter 4 contains a description of the research design that was used to test the hypotheses. In Chapter 5, the results of the experiment are presented and discussed, and in Chapter 6, the contributions, limitations and possible extensions of this research are enumerated.

CHAPTER 2
REVIEW OF RELEVANT RESEARCH

Overview

The tax compliance literature that deals with the effects of fairness perceptions and IRS contact on tax compliance is reviewed in this chapter. The chapter contains a discussion of the contributions, strengths, and weaknesses of that research stream as well as an analysis of how it relates to this research.

The literature is organized according to the primary emphasis of the studies to be discussed. The first section is an overview of the traditional economic model of tax compliance, its shortcomings, and related empirical research. The second section discusses research that has examined the connection between perceptions of tax law fairness and compliance. Third, studies that have examined the tax compliance effects of IRS contact are discussed. Next, studies that have extended the IRS contact literature by considering the relationship between perceived unfair treatment caused by IRS contact and tax compliance are reviewed. Finally, this literature review concludes with a general discussion of the prior literature in this area and the expected contribution of this research.[5]

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[5] A number of general reviews of tax compliance research have been published (e.g., Jackson and Milliron 1986; Cowell 1990).

The Economic Model of Tax Compliance

The traditional economic model of tax compliance was developed by Allingham and Sandmo (1972) and Yitzhaki (1974). In their model, they assume individuals have a fixed endowment of income and must choose the amount of income to report to the tax authority. According to the model, this decision depends upon four variables: the amount of income earned, the tax rate, the probability of audit and the severity of fines for not reporting income. Taking these variables into account, the taxpayer reports the amount of income that will maximize the expected utility of the tax evasion gamble. Not surprisingly, this model predicts that the amount of income reported increases as the probability of audit and the level of fines increase. However, the impact of changes in the tax rate and income depends upon the taxpayer's risk preferences.

The economic model of tax compliance has been subjected to extensive empirical testing. Three representative studies using IRS data were conducted by Clotfelter (1983), Witte and Woodbury (1985), and Slemrod (1985). These studies examined factors thought to affect expected economic benefits from noncompliance such as audit probabilities, the probability of detection, fines and opportunity. The common finding among these studies was that tax compliance was dependent upon the type of income earned by taxpayers. Taxpayers with income that could be readily concealed from the IRS were more inclined towards noncompliance.

The economic model of tax compliance has also been tested using experimental studies. Studies representative of this subset of the literature are those conducted by Friedland (1982), Jackson and Jones (1985) and Spicer and Hero (1985). Friedland (1982) found that the probability of detection was a more important determinant of tax compliance than the size of tax penalties. On the other hand, Jackson and Jones (1985) found the opposite result: the size of tax penalties was more important than audit probabilities. Finally, Spicer and Hero (1985) discovered that taxpayers' estimates of the probability of detection increased with taxpayer contact with the IRS, resulting in a diminution of tax evasion.

There is a second branch of tax compliance literature based upon fiscal psychology that has sought to test the effects of noneconomic variables on tax compliance. The impetus for this research came from the realization that individual behavior does not seem to be driven by economic factors alone. Rather, researchers in this area have claimed that individual attitudes and characteristics such as morality, fairness perceptions of the tax system, complexity, and knowlege of peer behavior also affect taxpaying behavior. The subset of this body of research that is relevant to this study is reviewed in the next section.

The Effects of Distributive Fairness on Tax Compliance

On of the goals of the Tax Reform Act of 1986 was to improve the fairness of the tax code. In its explanation of the Tax Reform Act (TRA) of 1986, the Joint Committee on Taxation included the following discussion regarding tax fairness:

> A primary objective of Congress was to provide a tax system that ensures that individuals with similar incomes pay similar amounts of tax. The ability of some individuals to reduce their tax liability excessively under prior law eroded the tax base and required tax rates to be higher than otherwise would have been necessary. Congress was concerned that other individuals, unable to take advantage of tax shelters, had lost confidence in the tax system and may have responded by evading their tax liability.

Some, but not all, research findings support the assertions of the Joint Committee concerning the possible effects of perceived unfairness of the tax laws on taxpayers' behavior. Etzioni (1986) shows that perceptions of unfairness and noncompliance are correlated using time series data. Etzioni was not, however, able to establish that a causal link exists between the two variables. In addition, the Yankelovich, Skelly, and White, Inc. study (1984) found that the most frequently selected rationale (25 percent) for why people cheat on their income taxes was that people feel the tax system is unfair. The issue of tax fairness and its effects on taxpayer attitudes and compliance has been studied using both survey and experimental approaches. This line of research has been directed at distributive justice issues such as the fairness of an individual's tax rate vis a vis the tax rate applicable to another individual.

Equity Theory

Many of the research studies that examine the effect of perceived tax fairness on tax compliance use equity theory to explain and predict how taxpayers are affected by inequity in the tax system. The theory evolved from the theory of cognitive dissonance which predicts that individuals will take action to reduce the level of internal dissonance they experience when their cognitions are in disagreement. According to the theory, the major components of an exchange relationship are inputs and outcomes (Adams 1965). In a taxpaying context, equity theory has been applied by viewing taxes paid as inputs and services received from the government as outcomes.

Equity theory suggests that individuals use these outcomes and inputs to arrive at their perceptions of equity (Walster et al. 1978). The theory predicts that individuals will weight their outcomes and inputs according to their relative importance to the individual and that inequity is perceived to exist whenever a person's ratio of outcomes to inputs is not equal to some referent other's ratio of outcomes to inputs.

The second part of equity theory predicts how an individual will react once an inequity has been perceived. The theory predicts perceived inequity will create tension in the individual and that this tension will be in proportion to the magnitude of the inequity perceived. Also, this tension will motivate the individual to reduce the level of tension by some action to restore equity. In a taxpaying context, the theory predicts that taxpayers will attempt to restore equity by withholding tax payments from the government.

Survey Research

The survey research in this area has produced conflicting results. Spicer and Lundsted (1976) asked survey respondents questions concerning factors that they thought would be related to noncompliance. From the responses to these questions, they constructed a fairness index to capture individual attitudes toward the fairness of the tax laws. Also, a tax resistance scale measuring the propensity to evade taxes and a tax evasion index measuring self-reported past tax evasion were computed. Of all the variables that the researchers believed were related to noncompliance, the inequity variable was found to be the most predictive of individual scores on the tax evasion index and the tax resistance scale.

Using survey respondents from North Carolina, Song and Yarbrough (1978) studied the ethical beliefs of taxpayers with respect to the federal tax system. Responses to questions with an ethical dimension were tabulated to produce a tax ethics score for every respondent. The responses to other questions designed to measure variables hypothesized to be related to tax ethics were analyzed in conjunction with the tax ethics score to determine which, if any, of the independent variables were correlated with tax ethics. The researchers discovered that a variable representing belief that the tax burden is fair was positively correlated with individual tax ethics. They interpreted this finding to mean that those who believe the tax burden is shared fairly are more likely to make ethical tax reporting decisions.

In a survey of Oklahoma city taxpayers, Scott and Grasmick (1981) asked questions designed to measure the relationship between compliance with tax laws and perceived equity. Perceived equity was measured using six questions that asked about the fairness of each taxpayer's exchange with the government--the exchange being the taxes that are paid for government services received. Overall, the correlation between noncompliance and perceived unfairness was found to be weak. However, for a subset of the respondents, those individuals with low moral values and sanction perceptions, the correlation between perceived unfairness and tax evasion was much stronger.

A survey of Oregon taxpayers was conducted in 1984 to determine the relationship between beliefs and self-reported noncompliance (Mason and Calvin 1984). Rather than ask respondents about federal taxpaying behavior, the researchers asked about compliance with the Oregon state income tax. Although this would seem to limit the generalizability of their results, the researchers maintain that compliance with Oregon state income tax laws is a good surrogate for federal taxpaying behavior since the definition of taxable income for state income taxes in Oregon is the same as for federal taxable income. Mason and Calvin failed to find a significant, direct relationship between noncompliance and perceived unfairness of the tax laws. However, they did find that perceived unfairness of the tax laws may cause taxpayers to be inclined towards evasion; nevertheless, they discovered that fear of getting caught mitigated this inclination.

In a survey that measured eighteen variables believed to be correlated with tax compliance, Porcano (1988) included one independent variable to measure perceptions of fairness of the exchange with government where taxes are traded for services. Using discriminant analysis, the researcher found that this measure of tax fairness was a significant predictor of admitted past tax evasion but not of hypothetical future evasion.

Experimental Research

The relationship between tax compliance and the perceived fairness of the tax structure has also been studied using experiments to overcome the methodological limitations inherent in the survey method. However, like the results obtained using surveys, the experimental evidence is also mixed.

In an early experiment testing the effects of perceived inequity on compliance, subjects who perceived they were taxed at different rates than were other subjects having the same amount of income manifested differences in their levels of tax compliance within the experiment (Spicer and Becker 1980). Specifically, those subjects who perceived they had a high tax rate relative to other participants in the experiment exhibited lower levels of compliance than those subjects who perceived they had relatively the same tax rate or who perceived they had a relatively lower tax rate.

In a later experimental study, students were given various hypothetical tax reporting scenarios and asked if they would evade the payment of taxes (Kaplan and Reckers 1985). While the primary purpose of the study was to measure the effects of penalties and the probability of detection on tax evasion, the researchers also gathered data on perceived fairness of the taxes. Subjects' responses to the hypothetical tax payments situations were unaffected by perceived fairness.

Another study examined the possibility that taxpayer education could be used to increase tax compliance by mitigating perceptions of tax inequity (Harris 1989). In this study, Harris found that individuals' perceptions of the fairness of the tax system can be changed through education, and his results indicate that increases in the perceived fairness of the tax system lead to increased compliance.

A recent paper examined how the availability of tax shelters to high income taxpayers affects perceptions of fairness and compliance (Hite 1990). Rather than study the effects of horizontal inequity on attitudes and behavior as Spicer and Becker (1980) did, Hite tested the relationship between perceived vertical inequity, attitudes, and compliance. Specifically, her experimental manipulation consisted of two levels involving scenarios that described tax shelters. In the victim scenario, subjects were told that someone with a tax shelter making three times their income paid the same amount in tax. In the beneficiary scenario, the subjects were informed that, because of their involvement in a tax shelter, they would pay the same tax as their neighbor who earns three times less. After receiving this information, subjects were asked how much of a recent financial windfall they would report on their tax returns. Tax compliance was measured using the amount of income reported, and perceived fairness of the tax scenarios was measured using a fairness scale.

Hite (Hite 1990) found that tax shelters were not perceived as inequitable. However, when she controlled for prior participation in tax shelters, she did find that those who had not participated in tax shelters perceived tax shelters as inequitable. Accordingly, prior experience was significantly related to the perceived inequity of tax shelters. However, using only those subjects who had not participated in tax shelters, Hite failed to find a significant relationship between inequity perceptions and compliance behavior both for victims and beneficiaries.

The Effect of IRS Contact on Compliance

Research involving the effects of IRS contact on tax compliance has attempted to determine whether IRS contact influences taxpayer behavior primarily through its effect on variables included in economic models of tax compliance or through its effect on a fiscal psychology variable. The theory that underlies much of this research comes from Strumpel's (1969) model of tax enforcement. Essentially, his model predicts that rigorous tax law enforcement produces two opposing forces: a deterrence effect caused by the fear of sanctions and an alienation effect caused by the aggressive nature of tax law enforcement. According to this theory, IRS contact would increase taxpayers' subjective probabilities of audit and detection thereby, producing a deterrent effect as predicted by economic models of tax compliance. Simultaneously, if the IRS contact is perceived to be coercive or unpleasant, taxpayers' willingness to contribute to the government through tax payments would be diminished. However, the theory is silent as to which of the predicted effects will dominate.

In addition to the alienation effect predicted by Strumpel's model, researchers have proffered a second explanation as to why IRS contact could actually decrease tax compliance.

They suggest that IRS contact might produce an "experiential effect" whereby taxpayers learn how to evade more successfully after coming into contact with the IRS.

To date, the results from this line of research are quite divergent. Some results support the conclusion that greater contact results in lower compliance. For example, European countries with the strongest tax enforcement tend to also have the lowest tax compliance levels (Strumpel 1969). Also, survey research involving U.S. taxpayers has shown that those taxpayers who had previously been audited tended to have high levels of tax resistance and also higher levels of admitted tax evasion (Spicer and Lundstedt 1976). Such results are not totally unexpected given that taxpayers who evade are more likely to be audited. Other researchers have found that increased contact with the IRS leads to greater compliance (Witte and Woodbury 1985). This finding was corroborated by a lab experiment testing the effects of tax audits upon future compliance (Spicer and Hero 1985). This study showed that taxpayers who had been audited overestimated the probability of a future audit, thus decreasing the amount of future noncompliance.

In a recent study, Sheffrin and Triest (1992) model compliance as a function of various attitudes and the perceived probability of detection. Further, they model the perceived probability of detection as a function of past contact with the IRS and of knowledge of anyone having past difficulties with the IRS. The researchers test their models using data from a survey commissioned by the IRS and conducted by Lou Harris and Associates (1988). They find that knowing someone who has had past difficulties with the IRS produces a significant decrease in perceptions of the probability of detection. In addition, they find that the decreases in the probability of detection are significantly related to increases in noncompliance.

Sheffrin and Triest interpret these results to mean that increased enforcement reveals to taxpayers how easy it is to get away with evasion, which may lead to increases in noncompliance. However, they also acknowledge there may be another explanation for the results they obtain--direct and indirect contact with the IRS may negatively affect fairness perceptions of the tax enforcement process, which, they suggest, may reduce compliance. Unfortunately, the question in the survey used to determine if respondents had knowledge of anyone who has had difficulties with the IRS was not specific enough to determine if respondents perceived that the difficulties were caused by unfair treatment. Thus, it is impossible to know which of the two interpretations truly explains the observed relationship between IRS contact and noncompliance.

In addition to using survey and experimental data, researchers have used actual IRS taxpayer data to study the relationship between IRS contact and tax compliance. Using such data, Hunter (1987) studied the impact of IRS enforcement actions upon subsequent taxpayer compliance. To accomplish this, he compared a group of taxpayers who received either a notice of levy, a notice of federal tax lien, or a seizure notice to two separate control groups. The first control group was composed of taxpayers who were selected as they were about to receive a third notice demanding payment for additional taxes owed. At the time they were selected, members of this first control group had not been exposed to any enforcement action other than having received two notices requesting payment. The second control group was made up of taxpayers who also had received two notices requesting payment of additional taxes owed. However, members of this control group did not receive a third notice demanding payment until two years after members of the first control group received their third notice. The two control groups differed in terms of the amount of enforcement action they had experienced at the time the study was conducted.

The comparison between the enforcement group and the two control groups was conducted using data from the first full tax year after enforcement had taken place. By this time, members of the first control group may or may not have been exposed to enforcement action depending upon the resolution of their cases. However, because of the two year lag between receiving the second and third notices for payment, members of the second control group had not been exposed to any additional enforcement actions prior to the time the groups were compared.

The results of this study indicate that taxpayers who received a lien, levy, or seizure notice had higher noncompliance rates one year after the enforcement activity than the two control groups. Specifically, they had a greater percentage of noncompliance as measured by taxpayer delinquent accounts and balance due accounts. Although the results of this study are subject to varying interpretations, the author suggests that the results may reflect a feeling held by members of the enforcement group that they were treated unfairly.

Both Sheffrin and Triest (1992) and Hunter (1987) suggest that taxpayers' perceptions of the fairness of the tax enforcement process may have produced the results they obtained. However, both studies measure IRS contact very broadly, without separately measuring fairness perceptions produced by the IRS contact. For this reason, there remain three alternative explanations for the results they obtained. First, taxpayers may have lowered their perceptions of detection risk after being contacted by the IRS. Alternatively, the taxpayers may have felt alienated as a result of the aggressive nature of the IRS enforcement, or they may have felt that they were treated unfairly by the IRS.

The Effects of Procedural Fairness on Tax Compliance

The IRS contact literature previously described does not consider the perceived fairness of IRS enforcement to affect tax compliance, insofar as unfair enforcement differs from aggressive enforcement. Also, the tax literature examining the effects of distributional fairness on tax compliance deals with the perceived fairness of tax structure variables such as tax rates rather than the perceived fairness of the enforcement process. A third research stream combines the focus of the IRS contact literature and the fairness literature. Studies in this area have attempted to determine if taxpayers' fairness perceptions of the tax enforcement process developed through either direct or indirect contact with the IRS affect taxpaying behavior. Unlike the distributional fairness literature, studies in this area have focused on the fairness of the tax enforcement process as opposed to the fairness of the tax code. To accomplish this, some researchers in this area have used procedural justice theory to develop the hypotheses they have tested (Kinsey 1992, McGraw 1989, Smith 1992).

Procedural Justice Theory

In procedural justice theory, emphasis is placed on fairness judgements of enforcement procedures and on the behavioral and attitudinal effects of such judgements. In contrast, equity theory describes how individuals judge and react to the fairness of outcomes. Procedural justice theory predicts that perceived procedural injustice indirectly causes disobedience to laws by reducing the perceived legitimacy of authority. Further, procedural justice theory describes the criteria that individuals use to judge the fairness of procedures. These criteria are consistency, bias suppression, accuracy, correctability, representativeness, and ethicality (Leventhal 1980).

Clearly, concerns involving the fairness of tax law enforcement fall within the domain of procedural justice theory. Although procedural justice has been studied for some time in other social settings (Lind and Tyler 1988), it has only recently been studied in tax contexts. While this theory has been confirmed in studies of compliance with other laws (Tyler 1990b), studies of procedural justice effects on tax compliance using survey data and IRS taxpayer compliance data have produced mixed results.

Survey Research

Most of the procedural justice research in the taxpaying setting has been done using a survey approach. Unfortunately, the results of these studies are limited by the methodological problems inherent in using survey data to measure tax reporting behavior. First, determining causality is difficult using survey data because any relationship found between procedural justice and tax compliance may be due to procedural justice concerns either being used as a rationalization for noncompliance or being a cause of noncompliance (Alm 1991). Second, many of the survey studies that include procedural justice perceptions as independent variables in tax reporting choice models include as dependent variables taxpayer reports of past or expected future compliance. Because individuals may be unwilling or unable to report their past or future tax reporting decisions (Klepper and Nagin 1989), there is a reliability problem measuring past and future tax reporting behavior.

In a large scale survey study conducted for the IRS by Yankelovich, Skelly, and White, Inc. (1984), the relationships among taxpayer attitudes and noncompliance, both behavioral and attitudinal, were investigated. By asking respondents several questions concerning the honesty and fairness of the IRS and its employees, taxpayer attitudes towards the IRS were measured. Once measured, these perceptions were included as an explanatory variable in a regression model relating various demographic and psychological variables to compliance norms and reported noncompliance. The researchers concluded that there was no relationship between attitudes about the procedural fairness of the IRS and actual tax cheating or the acceptance of tax cheating. Nevertheless, the two least compliant subgroups of taxpayers studied, those that rationalize noncompliance and those view noncompliance as a game, were less likely to believe the IRS was fair and honest than subgroups that were more compliant. This could be interpreted to mean that noncompliant taxpayers claim that the IRS is less than fair in order to rationalize their own behavior.

Smith (1987) made use of survey data from the Yankelovich, Skelly, and White, Inc. (1984) study to investigate a related hypothesis dealing with the relationship between perceptions of the IRS and tax compliance. Smith hypothesized that the quality of service provided by the IRS has an effect upon taxpayer commitment to comply and actual tax compliance. Perceptions of the quality of IRS service were measured using an attitudinal scale made up of twelve questions asking about various dimensions of IRS service. For example, one of the questions was about the willingness of IRS employees to help while another was about the time it took to get information. He found that individuals who perceive the IRS provides quality service are less likely to condone tax evasion. However, Smith did not find that perceptions of IRS quality service are directly related to admissions of underreported income. Rather, he found that IRS quality service affects compliance indirectly through its effect on taxpayers' attitudes concerning the acceptability of tax cheating, which, in turn, were strongly related to actual noncompliance.

Porcano (1988) used discriminant analysis to determine the relative contribution of eighteen demographic and psychological variables towards explaining reported past and future tax compliance behavior. Among the independent variables, Porcano included a variable for perceptions of procedural justice that was measured by asking survey respondents three questions. Two of the questions asked about the degree of input taxpayers have in the tax lawmaking process. The third question asked if the IRS used fair enforcement procedures. With respect to the procedural justice variable, Porcano found that procedural justice perceptions are significant discriminators of evaders from nonevaders when past evasion is used as a dependent variable; however, the effect was not in the hypothesized direction. Those who evaded taxes had a stronger belief in the procedural fairness of the IRS than those who were inclined to comply. Finally, procedural justice perceptions were not significantly related to intended future evasion.

Porcano's measurement of procedural justice perceptions is imprecise. He aggregated perceptions about the fairness of the legislative process with perceptions concerning the fairness of the IRS to obtain his procedural fairness variable. Thus, it is impossible to determine which of the two forms of procedural justice are responsible for the results obtained.

Before McGraw (1989), the research conducted in this area was exploratory and not supported by theoretical underpinnings. McGraw (1989) was the first to recognize that procedural justice theory might have some explanatory power in the tax domain and to apply the theory in producing testable hypotheses. Using a survey approach, she asked 127 taxpayers who had been contacted by the IRS to answer several questions regarding their treatment by the IRS. To measure individual perceptions of procedural justice, respondents were asked to evaluate how willing the IRS employee had been to listen to their side of the story. Also, these same individuals were asked to give an overall evaluation of the performance of the IRS during the time they had dealings with the IRS. McGraw found a significant correlation between perceptions of procedural justice and evaluations of IRS performance. Her results suggest that taxpayers consider procedural fairness when evaluating the quality of IRS activities.

McGraw's results are important in that they show that procedural justice concerns exist in the tax domain; however, they are limited in the sense that do not indicate what effect perceptions of procedural justice have upon tax compliance. They only apply to evaluations of the quality of the IRS. Also, McGraw's measure of procedural justice, the ability to have a voice in the settlement of tax matters, is only one of several dimensions of procedural justice mentioned earlier that could affect the attitudes and behavior of taxpayers.

In an extension of his earlier paper, Smith (1992) used data from a 1987 (Harris and Associates 1988) survey commissioned by the IRS to examine the effects of reciprocity, procedural justice, and IRS contact with taxpayers on taxpayer commitment to tax compliance and past underreporting of income. Specifically, Smith postulated that taxpayers will react to greater IRS responsiveness and efficiency by being more compliant with the tax laws. This prediction is based upon previous research in social psychology, which has shown that individuals will reciprocate when concessions are made to them (Bardach and Kagan 1982). Similarly, Smith posited that taxpayers who feel the IRS treats people fairly will be more compliant with the tax laws. This hypothesis was drawn from procedural justice theory in social psychology. As Smith conceded, the two constructs, reciprocity and procedural justice, may be empirically indistinguishable although they are theoretically distinct.

Smith's results support some of his predictions. He found that reciprocity was related to perceptions of procedural justice. However, he also discovered that reciprocity did not add any additional explanatory power beyond the procedural fairness variable to his model of attitudes and behavior. Perceived procedural fairness was found to be significantly related to taxpayers' feelings concerning the acceptability of underreporting. As the perceived procedural fairness of the IRS declined, underreporting became more acceptable. However, the relationship between perceived IRS procedural fairness and the self-reported likelihood that income had been underreported in the five years prior to the study was not significant. According to these findings, the perception of IRS procedural injustice negatively affected normative commitment to tax compliance but not self-reported past behavior.

In addition to reciprocity and procedural justice, Smith included taxpayer contact with the IRS as an exploratory variable in the model he tested. Unlike the procedural justice variable, taxpayer contact was not intended to measure fairness perceptions of the IRS resulting from IRS contact. Rather, it measured whether subjects had ever been contacted by the IRS and, if so, the outcome of the contact.[6] Smith's results showed that taxpayer contact was not significantly related to a taxpayer's normative commitment to tax compliance or to a taxpayer's past behavior.

Smith's research is valuable because it documents a relationship between perceptions of IRS procedural injustice and taxpayers' commitment to compliance. Nevertheless, there are problems with his research that result from the nature and source of the data he analyzed. Specifically, the sparseness of the data Smith uses makes it difficult to obtain measures that adequately represent the theoretical constructs underlying the independent variables in his model. Although the survey instrument used to collect the data Smith analyzed contained several questions related to procedural fairness, there were not enough sufficiently detailed questions to be able to capture more than a general measure of perceived procedural fairness. Smith's variable for procedural fairness was determined using a scale based upon the responses to four questions asking about the fairness of IRS procedures, the integrity of IRS employees, the accuracy of IRS tax records, and the likelihood that the IRS would demand more money than they should. While Smith's measure of procedural fairness is probably satisfactory as an overall measure of perceived procedural fairness, it does not allow Smith to measure the separate effects of the various dimensions of procedural fairness, which have been found to differ in strength according to context (Barrett-Howard and Tyler 1986). Such information might be useful in documenting which of the procedural justice criteria apply in a taxpaying setting.

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[6] There were three possible outcomes which were examined. These outcomes were as follows: no additional tax due, additional taxes due, or a refund due.

Like perceptions of procedural fairness, taxpayer contact is also measured imprecisely. Taxpayer contact is a variable in Smith's model, which can take on the values 0, 1, or 2 depending on the number of different types of IRS contact the individual has had. The possibilities include no contact, contact with the payment of more taxes as the result, and contact with a refund as the result. Unfortunately, this variable does not allow Smith to discriminate between IRS contacts that were perceived to be procedurally fair from those that were not perceived that way. For this reason, Smith cannot separate the two possible countervailing effects of IRS contact: the deterrence effect and the procedural fairness effect.

Even without this problem, the paucity of respondents having direct IRS contact probably skewed Smith's results. Because most taxpayers have not had personal encounters with the IRS, taxpayer perceptions of procedural justice gathered using surveys tend to reflect attitudes that have been formed as the result of indirect rather than direct experience with the IRS. These indirect experiences happen when individuals are exposed to the media and when they have conversations with friends and acquaintances who have had direct contact with the IRS. Of all the respondents to the survey Smith used, approximately eighty-three percent of them had not had any contact with the IRS. Hence, Smith's results are probably driven by the perceptions of those respondents who had only indirect contact with the IRS. If taxpayers' perceptions are invariant to the form of IRS contact experienced, this would not be a problem. However, Smith suggests this is probably not the case. Therefore, Smith's results are limited in scope because they neither show how those having direct contact with the IRS are affected by the perceived procedural fairness of that contact nor do they show what the differential effects of direct and indirect IRS contact upon attitudes and behavior might be.

One of Smith's dependent variables, the probability of underreporting income, was obtained from a question asking respondents about the probability that they had underreported income during the last five years. However, the questions used to measure the independent variables measured norms and perceptions at the time the survey was conducted. This is problematic because noncompliance is assumed to be influenced by these norms and perceptions, yet the temporal measurement of the variables is consistent with the opposite view--that attitudes and norms measured after the noncompliance has occurred are rationalizations rather causes of noncompliance. This measurement problem compounds the problem of establishing causality that is already present due to the use of survey data.

Kinsey (1992) investigated the possibility that, in addition to deterrence effects, IRS contact with taxpayers increases the likelihood of tax cheating. To test her hypotheses, she used separate regression models to determine how personal contact with the IRS, vicarious contact with the IRS, admitted past noncompliance and other control variables affect sanction perceptions, attitudes concerning the fairness of tax laws and the likelihood of future tax cheating.

Kinsey's results suggest that the effect of perceived IRS procedural injustice differs according to whether the contact is personal or vicarious. Specifically, Kinsey found that direct contact with the IRS that causes the perception that more tax was paid than was legally owed reduced the likelihood of future cheating. Conversely, vicarious contact with the IRS through someone who feels they have paid the IRS more than was legally owed increased the likelihood of future cheating.

Kinsey's ancillary findings provide one explanation for differing effects of the two types of IRS contact. First, her ancillary results show that taxpayers who are subject to higher rates of audit actively share information about IRS enforcement experiences. Second, these results indicate that those who have had negative experiences with the IRS have a greater propensity for telling their stories than those who have been treated fairly. Smith asserts that this bias produces more intense feelings of IRS injustice among those who have formed their attitudes from second-hand accounts of IRS enforcement efforts than among those who have personally experienced the IRS contact. This difference in attitudes, according to Kinsey, helps to explain why vicarious unfair IRS contact promotes future cheating whereas personal unfair IRS contact produces a deterrence effect.

Kinsey improves upon Smith's (1992) research by including separate variables for the type of personal and vicarious contact. Personal and vicarious contacts were categorized according to whether respondents perceived that more tax was paid than legally owed, less tax was paid than was legally owed, or the correct amount of tax was paid. In theory, this modification to Smith's model should have allowed Kinsey to measure and compare the effects of unfair personal and vicarious IRS contacts. In reality, it did not.

Kinsey's data comes from a survey with a total of 1,202 respondents. Of that number, only 177 indicated that they had been contacted during the past three years concerning a problem with their tax returns. Further, only 19 of the 177 reporting personal contact indicated that they had paid more tax than was legally owed as a result of this contact. Of these 19, it is likely that few, if any, would have the opportunity to escape detection of underreported income given that most of the 177 with personal contact appear to have received computer generated notices rather than audits of returns. This suggests that the significant deterrence effect of unfair personal IRS contact is probably driven by taxpayers who don't have the opportunity to cheat. Kinsey, using the results from a supplemental analysis, arrives at the same conclusion. She indicates that the deterrence effect of personal IRS contacts is concentrated among those taxpayers who had received IRS notices about unreported income. In contrast, she finds in a separate supplemental analysis that the relationship between unfair vicarious contact and greater future cheating is driven almost entirely by taxpayers with the ability to underreport income without being detected. Therefore, it seems there is an alternate explanation for the differing effects of personal and vicarious contact that Kinsey documents. Perhaps, unfair personal IRS contact did not result in an increased likelihood of future cheating simply because few if any of the subjects with unfair personal contact would have the opportunity to cheat in the future and remain undetected while many of the respondents with unfair vicarious contact did have this opportunity. If this is so, Kinsey's findings concerning personal contact are not generalizable to those who have the opportunity to cheat. Further, they should not be compared with her findings concerning the effect of unfair vicarious IRS contact.

In addition to this limitation, Kinsey's results are subject to one of the limitations mentioned earlier in discussions of the McGraw (1989) and Smith (1992) studies. Her measure of procedural fairness, whether more tax was paid than was legally owed, is crude and does not consider the various components of procedural justice that have been shown to matter in the procedural justice literature.

Besides the problems of accurately measuring tax reporting behavior and determining causality, there are other limitations with these studies that have used surveys to measure procedural justice effects on tax compliance. Because the questions that were used to measure perceptions of procedural justice were general in nature or only addressed one dimension of procedural fairness, the results of these studies only apply to overall perceptions of procedural justice. They do not show which of the multiple criteria of procedural justice or what combination of these criteria matter in the tax domain. Further, these studies each use different questions to measure the same underlying construct--procedural justice. For example, McGraw (1989) asks if the IRS listens to measure perceptions of procedural justice whereas Kinsey (1992) asks if more taxes were paid than owed. This lack of a consistent, comprehensive measure of procedural justice limits the comparability and generalizability of these studies. Finally, because most taxpayers have not had contact with the IRS, individuals who respond to surveys typically have had vicarious contact with the IRS rather than direct contact. For this reason, the effects of procedural justice perceptions on tax compliance that some of the survey studies above document are probably caused by perceptions created through vicarious contact with the IRS. Thus, the question of whether direct unfair IRS enforcement reduces tax compliance remains unanswered.

Quasi-Experimental Research

An in-house IRS study was conducted to determine if unfair tax enforcement procedures would cause an increase in the level of noncompliance subsequent to the IRS enforcement activity (Colson 1989). Colson extends Hunter's (1987) prior study using IRS data to examine how the quality of tax law enforcement affects the level of tax compliance.

To accomplish this, delinquent accounts that had been assigned to revenue agents for collection were selected. The resulting sample was categorized into two groups according to the presence or absence of an error in enforcement. This determination was made by examining quality control checklists that had been prepared to monitor the auditor's performance. On the checklist, an error was defined as a required decision that had not been made or a required action that had not been taken. To determine if the presence of enforcement errors caused a difference in compliance between the two groups, tax filing information for the period subsequent to the enforcement actions was compared.

The results of this comparison indicate that there was no difference in subsequent compliance between the two groups. While interesting, the results of this study are not general enough to conclude that tax law enforcement has no effect upon subsequent compliance. Colson's definition of an error in enforcement does not differentiate between the type of errors that were made. Further, it includes only errors of omission but not those of commission. Finally, Colson's sample only includes hardcore noncompliers and does not account for compliers who may be pushed into noncompliance by what they perceive to be unfair enforcement.

Discussion and Conclusion

Much of the literature related to the effects of tax fairness and IRS contact on taxpayer compliance is tangential to the research hypotheses to be tested in this dissertation. The tax fairness literature dealing with tax compliance looks at tax compliance effects from perceptions of tax law fairness rather than from perceptions of procedural justice within the tax system. The literature that has examined the effect of IRS contact on compliance has, for the most part, overlooked procedural justice perceptions as a possible explanation for reductions in tax compliance. Rather, lower tax compliance from IRS contact is, according to the assertions in the IRS contact literature, caused by either an alienation effect or experiential effects whereby taxpayers modify their perceptions of detection risk as they come in contact with the IRS. By controlling for these effects, the approach used in this research will disentangle these effects from the compliance effects of procedural justice perceptions.

The research methodology and hypotheses in this work were selected to avoid the methodological problems inherent in the prior procedural justice research in the tax area and to answer questions, which to date, remain unanswered. Much of the prior research on the tax compliance affects of procedural justice perceptions is limited by measurement problems and problems with establishing causality. These problems are circumvented in this research by using an experimental approach. This approach will also make it possible to measure the effects of perceived procedural injustice stemming from direct IRS contact, which has not been adequately addressed in the prior research. Finally, this dissertation will add to the existing literature, by using a theory driven approach to test for the separate compliance effects of two dimensions of procedural justice: consistency and accuracy. Prior research in this area has failed to recognize that procedural justice is a multidimensional construct.

CHAPTER 3
THEORY AND HYPOTHESES

Introduction

This chapter begins with a discussion of a general model of tax compliance. Then, this general model is expanded to include procedural justice theory. As part of the discussion of procedural justice theory, the theoretical characteristics of procedural justice and the effects of procedural justice perceptions on attitudes and behavior will be explained. Also, several hypotheses concerning the effects of procedural justice judgments in a tax enforcement setting will be proposed. Finally, the chapter concludes by introducing reactance theory, which supplements procedural justice theory to explain differences in the magnitude of individual reactions to tax enforcement activities.

In addition to economic models of compliance that were discussed earlier, researchers have proposed other, more general models of tax compliance that also include psychological variables such as taxpayer's fairness perceptions and personal ethics (Smith and Kinsey 1987, Weigel et al. 1987). One of the proposed models includes taxpayer's fairness judgments about tax authorities as a variable that influences tax evasion behavior (Weigel et al. 1987). However, this model is too general in that it fails to specify how taxpayers make fairness judgments. Also, it does not identify how fairness judgements are linked with behavior. By supplementing the general model with procedural justice theory, these questions can be answered and specific hypotheses concerning the affects of procedural justice in a tax setting can be proposed. The next section explains this general model of tax evasion behavior to show how procedural justice judgements are situated within the model and to show how procedural justice theory can augment the model.

General Model of Tax Evasion Behavior

Weigel et al. (1987) have constructed a social psychological framework of tax evasion that models tax evasion behavior as the result of interacting social and psychological conditions. Figure 3-1 contains a graphical representation of their model. According to their model, a taxpayer's particular social setting produces two situational instigations towards tax evasion and three countervailing situational constraints against tax evasion. Situational instigations consist of financial strain and social norms while social constraints consist of opportunity, legal controls and social controls. These social instigations and constraints are included in the model to explain variances in tax evasion behavior caused by environmental differences among specific taxpayer groups. However, to explain differences in individual behavior within such groups, the model also includes personal instigations and constraints that are individual analogues of the social instigations and constraints. The two personal instigations in the model are personal strain and personal orientation, and perceived opportunity, perceived risk of punishment, and intolerance of tax evasion are the personal constraints in the model. Unlike the social instigations and constraints, these variables are constructs for individual perceptions, orientations, and attitudes. In theory, they interact with their situational analogues to either exacerbate or mitigate tax evasion behavior.

In the model, two situational instigations mediated by two concomitant personal instigations combine to push taxpayers toward tax evasion behavior. The first situational instigation, financial strain, is produced when an individual is confronted with a large, unexpected tax bill. However, the model predicts that individual responses to this situational instigation will be diverse and that they will depend upon the degree to which this situational variable is either magnified or attenuated by personal strain, the personal instigation linked with financial strain in the model. Examples of personal strains would include individual perceptions of the difficulty of making an unexpectedly large tax payment and personal feelings that the amount of tax owed is excessive because of unfair tax laws or unfair tax authorities. Personal strains like these increase the likelihood of tax evasion given the level of financial strain. Conversely, an absence of a personal strain or a diminished level of a personal strain when an individual, for example, believes the tax laws are fair would tend to reduce the likelihood of tax evasion when financial strain is present.

In addition to financial strain, social norms are expected to act as a situational instigator. According to the model, individuals exposed to a social climate where personal wealth is the yardstick by which success and status are measured are more likely to engage in tax evasion than an individual not exposed to this type of climate. Again, differences in individual response to this situation are due to a personal instigation that interacts with the social context to determine whether the social norms are internalized and acted upon. This second personal instigation is an individual's tendency to be either self-serving or to be community-oriented.

To counterbalance the situational and personal instigations described above, the model includes three situational and personal constraints. One situational constraint, the opportunity for evading taxes, varies among taxpayer groups because of differences in withholding or in the level of cash income. This variability in opportunity to escape punishment for tax evasion helps to explain differences in tax compliance among taxpayer groups. Nevertheless, within these groups, differing individual perceptions of the opportunity to evade (a personal constraint) causes individual differences in behavior. Similarly, the probability of getting caught and being punished for tax evasion is a situational constraint that may vary within taxpayer groups because of differences in audit rates across geographical regions and occupations. This objective probability is tempered, however, by a another personal constraint: the subjective perceptions of individual taxpayers regarding the risk of punishment. Lastly, beliefs held by taxpayer's associates regarding the propriety of tax evasion are the final social constraint in the model, and personal ethics and morals are the analogous personal constraint. For example, an individual in a social setting where tax cheating is condoned or even extolled will have a higher probability of tax evasion, yet he may not engage in this behavior because his personal mores are not consonant with those of the social group.

This general model of tax compliance behavior recognizes that perceptions regarding the fairness of tax authorities may affect tax compliance behavior. However, the model is incomplete. For example, it does not specify how perceptions of procedural fairness are linked to tax compliance behavior. Further, it does not indicate what criteria taxpayers apply when making judgements about the fairness of tax authorities. To predict and explain how taxpayers react to perceived injustice in the enforcement of tax laws, a more expansive theory must be used to augment the general theory of tax compliance described here.

The Theory of Procedural Justice

The Effect of Procedural Justice on Attitudes

Two examples of personal strain from the general model of tax evasion behavior described above, perceived fairness of tax laws and perceived fairness of tax authorities, have been studied to investigate how they affect tax compliance. Most of this research concerns taxpayers judgments of the fairness of tax laws and the effects of such judgements on taxpayer attitudes and compliance (Etzioni 1986, Harris 1989, Hite 1990, Spicer and Becker 1980, Yankelovich 1984). This research emphasizes judgements concerning the fairness of tax structure variables such as relative tax rates that affect the amount of tax paid. To explain how taxpayers make such judgements, researchers in this area use equity theory (Adams 1965), which describes how individuals judge and respond to the fairness of outcomes. However, to explain how taxpayers make judgements about the fairness of a process like tax enforcement, a justice theory that emphasizes process as opposed to outcome is needed.

The theory of procedural justice (Thibaut and Walker, 1975) was developed as the result of behavioral experiments designed to test reactions to dispute resolution procedures. Thibaut and Walker tried to determine if a litigant's perceptions of the fairness of the legal process varied according to the amount of process control possessed. Specifically, they compared the reactions of litigants under an adversarial system of justice with reactions under an inquisitorial system of justice. In the adversary system as implemented in the American and the British court system, disputants select their attorneys and control the presentation of evidence. Under the inquisitorial system, judges control the selection and presentation of evidence. Thibaut and Walker show that disputants perceived verdicts to be more fair when they were given process control even in those circumstances where the verdict was unfavorable. The finding that procedural fairness increases the level of satisfaction with even negative outcomes is pervasive in the procedural justice literature.

Researchers have also studied the effects of procedural justice in contexts other than dispute resolution. For example, procedural justice studies have examined the effects of process in the work environment (Greenberg 1987), in politics (Tyler et al. 1985), in criminal courts (Casper et al. 1988), in interpersonal relationships (Barrett-Howard and Tyler 1986) and in law enforcement (Tyler and Folger 1980). Typically, these studies have measured both outcome satisfaction and attitudes regarding the fairness of legal institutions and authorities.

Two of these studies that involve contexts similar to tax law enforcement are particularly relevant. The first study showed that citizens evaluate the fairness of their experiences with police in terms of procedure as well as in terms of outcome (Tyler and Folger 1980). A later study explored the same issue by surveying defendants in misdemeanor and traffic courts regarding their attitudes toward the outcome of their cases, the judge and the court system (Tyler 1984). Again, the results indicated that procedural fairness had a significant effect upon all three attitudes. Furthermore, procedural fairness was found to be more important than outcome in determining attitudes toward the judge and the court. This research supports the assertion that citizen's attitudes toward legal authority and institutions are influenced not only by outcomes received, but also by their perceptions of the fairness of the treatment received from authorities.

The Effect of Procedural Justice on Behavior

Procedural justice has been shown to affect not only attitudes towards the legal system but also compliance with laws. In an early experimental study, Friedland et al. (1973) studied the relationship between obedience to laws and three factors: fairness of procedures for monitoring compliance, severity of penalty for noncompliance, and the extent to which obedience to laws benefits the rule-making authority alone or both the authority and those who must obey the rules. The study found that noncompliance was higher when unfair surveillance procedures were employed. Also, subjects who were exposed to unfair monitoring tactics became more clever at cheating. Finally, these effects were found to be stronger when the authority alone was rewarded for participant's compliance with the rules. Using a different approach, McKewen and Maiman (1984) compared the level of compliance with mediated decisions to the level of compliance with adjudicated decisions in small claims court. Compliance was greater with those decisions that were perceived to be fair. While McKewen and Maiman did not directly measure perceptions of procedural justice, their data indicate that it was an important factor in the overall fairness assessment.

Later studies proposed hypotheses concerning the nature of the link between procedural justice and compliance with laws. For example, Tyler (1990b) hypothesized that the relationship between procedural justice and compliance is an indirect one. According to Tyler's hypothesis, perceptions of procedural justice within the legal system affect attitudes toward the legitimacy of authority that in turn affect compliance with the law. To test this assertion, Tyler surveyed the fairness perceptions of citizens concerning the entire legal system including the police. Interestingly, he found that perceptions of procedural justice (how the police enforced the law) exerted an independent effect upon citizens attitudes regarding legitimacy of authority apart from the perceived fairness of legal outcomes. Moreover, he found that perceptions of procedural justice had a larger effect upon legitimacy than favorability or fairness of outcome.

After testing the link between procedural justice and legitimacy, Tyler tested for a relationship between legitimacy and compliance. His results indicate that procedural justice perceptions have an independent effect upon future compliance with the law with legitimacy acting as an intermediary between the two. In summarizing the implications of his findings, Tyler asserts that compliance with laws can be improved simply by making procedures more fair.[7]

Procedural Justice Criteria

Early research in procedural justice was focused on one characteristic of procedural justice: process control (Thibaut and Walker 1975) which is the control over the judicial process given to litigants in legal disputes. Later, however, Leventhal (1980) argued that procedural justice is a component of perceived fairness in almost any allocation context. Realizing that process control would not adequately describe the quality of procedural justice in other settings, Leventhal (1980) and Leventhal, Karuza, and Fry (1980) expanded procedural justice theory by proposing rules that can be used as a standard to judge the fairness of specific procedures in any context.

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[7] A change in procedures would not necessarily produce a corresponding change in behavior since behavior is affected by perceived procedural fairness rather than by real procedural fairness. Contrary to Tyler's assertion, when a real change in procedural justice is not followed by a corresponding change in perceptions, no improvement in compliance with laws will be expected.

According to Leventhal, to be fair, procedures must have the following characteristics: consistency, bias suppression, accuracy, correctability, representativeness, and ethicality. First, consistency is the equal treatment of individuals and the equal application of rules over time. Second, bias suppression exists when decision makers involved in carrying out procedures do not have two specific biases. The first type of bias is present when decision makers have a vested interest in the outcome of their decision, and the second bias is present anytime decision makers base their decisions on prior views rather than on the evidence available to them. Third, accurate procedures are defined as those that result in high quality decisions because they are made with accurate information. Fourth, correctability refers to the possibility that bad decisions can be corrected through some grievance process or appeal procedure. Finally, representativeness requires that individuals have a voice in how procedures are adopted and implemented, and ethicality requires that procedures conform to moral and ethical standards.

The relative importance of these six criteria of procedural justice has been examined across various settings. Studies have found that consistency, and to a lesser extent, accuracy and representativeness are the most important of the six rules of procedural justice. Nevertheless, the pattern of results indicates that the relative importance of the procedural justice criteria is context specific. For example, Barrett-Howard and Tyler (1986) tested the applicability of Leventhal's procedural justice rules across sixteen different interpersonal allocation contexts. According to their findings, consistency was the main rule individuals used to measure procedural justice within the interpersonal contexts found in their experiment. More importantly, their findings showed that the criteria individuals use to judge procedural fairness as well as the relative importance of these criteria are not universal; rather, they differ according to the allocation context.

This difference in the individual application of the procedural justice rules is illustrated in a study conducted by Sheppard and Lewicki (1987). Using manager/employee relationships as a context to test the relative importance of Levethal's procedural justice criteria, they found that individuals used accuracy and representativeness, rather than consistency, to measure the fairness of procedures. Commenting upon the tendency of individuals to use different criteria to measure procedural justice in different situations, Tyler states: "individuals do not have a single schema of a fair process that they apply on all occasions. Instead, they are concerned with different issues in different circumstances" (1988, 131).

The Effects of Procedural Justice in a Taxpaying Context

Our tax system, made up of laws that are enforced by the IRS, is a mechanism to allocate the tax burden among taxpayers. Accordingly, the theory of procedural justice predicts that the manner in which tax laws are enforced by the IRS will affect the perceived fairness of the tax system and possibly also tax compliance. Presumably, these effects will occur when IRS audits or other IRS enforcement procedures violate the criteria of procedural justice. For example, failing to consistently enforce the tax laws across all taxpayers should result in diminished perceptions of fairness and possibly lower levels of tax compliance. Further, unwarranted requests for additional tax payments caused by inaccurate information, a violation of the accuracy criterion, should lead to the same consequences.

Consistency. Given that consistency has been an important criterion of procedural justice in other contexts, consistency may also be an important factor in determining taxpayer attitudes and compliance in the tax setting. This issue is relevant since taxpayers are treated inconsistently in our tax system. Some inconsistency exists because the IRS uses a statistical model based upon income level, occupation, and other variables to select taxpayers for audit. However, other inconsistencies are more difficult to explain. For example, audit rates have differed across individuals according to where they reside. According to Burnham (1990), 14 out of 1,000 returns filed during 1988 in the IRS's San Francisco district were audited as compared to only 5 out of 1,000 in the New Jersey district. Burnham also reveals that IRS districts have differed greatly in terms of how harshly taxpayers are treated. He notes that seizure rates in active IRS districts are thirty to forty times greater than seizure rates in other less active districts.

According to procedural justice theory, inconsistency in the enforcement of tax laws may affect a taxpayer's attitude toward the acceptability of tax cheating and his actual taxpaying behavior.[8] The predicted effects of inconsistency upon taxpayer's attitudes toward compliance and actual compliance are stated in the following hypotheses:[9]

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[8] This statement assumes that taxpayers have the necessary information to form perceptions about the degree of consistency within the tax enforcement system. This information may be received through the media or through social networks of taxpayers that, according to Kinsey (1992), transmit information concerning member's experiences with the IRS.

[9] All research hypotheses are stated in the alternative form.

> H1: The failure to enforce tax laws consistently across taxpayers will make taxpayers more accepting of noncompliance with the tax laws.
>
> H2: The failure to enforce tax laws consistently across taxpayers will increase the level of noncompliance with the tax laws.

Accuracy. In addition to treating taxpayers inconsistently, the IRS has routinely sent large numbers of incorrect deficiency notices to taxpayers requesting additional payment of taxes and penalties. The problem is partly due to inaccurate information provided by the IRS information returns matching program. A General Accounting Office study found that of 6 million notices and letters sent to taxpayers during 1987, 48 percent were "incorrect, unresponsive, unclear or incomplete" (1988). Further, the report noted that a significant number of these defective responses had "resulted in the assessment of incorrect tax and penalties."

In earlier procedural justice research, individuals have used accuracy as a measure of procedural justice (Sheppard and Lewicki 1987). Consequently, if accuracy is an important criterion of procedural justice in a tax context, enforcement actions against taxpayers based upon inaccurate information may have an adverse effect on a taxpayer's attitude toward the acceptability of tax cheating and on actual taxpaying behavior. These predictions are summarized in the hypotheses below:

> H3: IRS enforcement actions based upon inaccurate information will make taxpayers more accepting of noncompliance with the tax laws.
>
> H4: IRS enforcement actions based upon inaccurate information will increase the level of noncompliance with the tax laws.

Indirect injustice. The theory of procedural justice is sufficiently broad to encompass not only those situations where taxpayers personally experience procedural injustice at the hands of the IRS, but also circumstances where taxpayers have been exposed to instances of injustice inflicted upon others. Procedural justice, or the lack thereof, has been shown to affect compliance in two ways (Tyler 1990a). First, it affects outcome satisfaction which, in turn, affects compliance. Therefore, when individuals experience bad outcomes accompanied by procedural injustice, compliance with laws is reduced. Second, procedural injustice affects compliance indirectly through its influence on perceptions of legitimacy--regardless of outcomes. This indirect effect really means that compliance will, according to the theory, be affected by procedural injustice even in those instances where the observer has no personal stake in the outcome of the process observed.

Many taxpayers will never experience procedural injustice by the IRS. It is more likely that they will be exposed to instances of IRS procedural injustice through the news media or through conversations with others. Prior survey research indicates that individual's share information about their encounters with the IRS through social networks (Kinsey 1992). Further, this research indicates that this kind of exposure to IRS misdeeds will increase noncompliance with the tax laws.

According to the theory of procedural justice, taxpayers who experience IRS procedural injustice second hand will experience a change in attitudes and will be less compliant. These predictions are formally stated in the hypotheses below:

H5: Taxpayers experiencing IRS procedural injustice indirectly will become more accepting of noncompliance with the tax laws.

H6: Taxpayers experiencing IRS procedural injustice indirectly will increase their level of noncompliance with the tax law.

The Effects of Psychological Reactance on Taxpayers

The procedural justice literature has explored how individuals differ in their perceptions of procedural justice. For example, individual differences such as age, gender, income level and political views have been found to explain differences in perceptions of procedural justice (Lind and Tyler 1988). However, this literature has nothing to say about how individual differences affect the magnitude of individual's responses to perceived procedural injustice. On the other hand, psychological reactance theory, which complements procedural justice theory because it also predicts how individuals will respond to encounters with authority, describes how individual characteristics influence individual responses to attempts to enforce compliance with laws. Like procedural justice theory, reactance theory can be viewed as an appendage to the general model of tax evasion behavior described earlier because it adds another source of personal strain to the general model. That source of personal strain, psychological reactance, comes from actual or perceived threats to freedoms caused by encounters with the IRS.

Psychological reactance is a familiar behavior. The strategy of reverse psychology is predicated upon psychological reactance. Simply stated, psychological reactance is a counterforce that motivates a person to restore a freedom that is threatened or has been eliminated (Brehm and Brehm 1981). For example, if a child is told not to eat spinach, psychological reactance would motivate the child to eat the spinach anyway. In this instance, the child has had her freedom to eat spinach threatened which causes her to act to preserve that freedom by eating the spinach. Threats to freedom may be commands, attempts to persuade, or threats of punishment for noncompliance.

In a taxpaying context, psychological reactance theory would predict that some taxpayers will respond to attempts to enforce the tax laws by actually reducing their level of compliance or by not complying at all.[10] According to reactance theory, individuals may consider the ability to avoid negative outcomes or punishment for noncompliance as freedoms (Brehm and Brehm 1981). Therefore, taxpayers who have suffered negative outcomes or who have been punished because of encounters with the IRS may feel that they have lost a freedom. The feeling that a freedom has been lost is not enough to produce psychological reactance, however. Individuals must also feel the lost freedom was important. But, because individuals will not all place the same importance on the lost freedom, the amount of psychological reactance they experience will vary from person to person (Brehm and Brehm 1981). Therefore, taxpayers may not all feel the same amount of psychological reactance after encounters with the IRS because they may have different feelings about the importance of the freedoms they have lost, if they feel freedoms have been lost at all. Consequently, taxpayers may vary in how they respond to negative experiences with the tax enforcement system.

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[10] This assumes that taxpayers can avoid discovery. Brehm and Brehm (1981) note that individuals who believe that discovery is likely will give up, a behavior they refer to as "learned helplessness". Cuccia (1994) has previously studied the effects of pychological reactance in a tax compliance setting.

Because psychological reactance theory predicts that individual differences will affect the response of taxpayers to tax enforcement efforts, a measure of individual psychological reactance will be included as a covariate in the model described in the chapter that follows.

Summary

This chapter has presented a general model of tax compliance that incorporates various social and psychological variables. One of these variables, personal strain, recognizes that the likelihood of tax evasion is increased when individuals perceive that tax enforcement practices are unfair. The general model was supplemented with procedural justice theory to derive testable hypotheses concerning the effect of perceptions of procedural injustice in a taxpaying setting. Finally, psychological reactance theory was introduced to account for individual differences in response to encounters with tax enforcement authorities. The chapter that follows describes the experiment that was devised to test the hypotheses presented in this chapter.

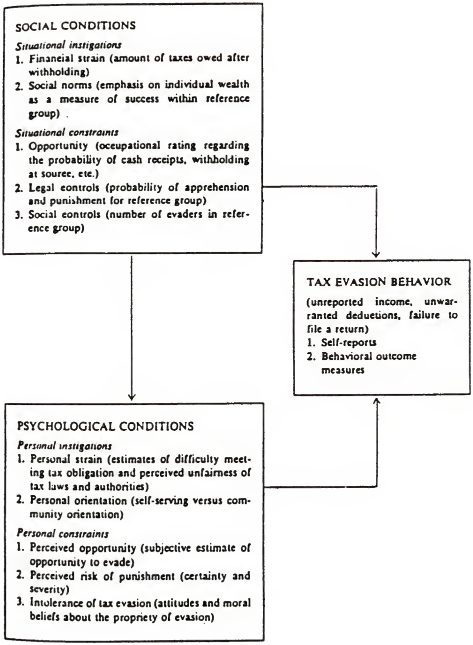
SOCIAL CONDITIONS
Situational instigations
1. Financial strain (amount of taxes owed after withholding)
2. Social norms (emphasis on individual wealth as a measure of success within reference group)
Situational constraints
1. Opportunity (occupational rating regarding the probability of cash receipts, withholding at source, etc.)
2. Legal controls (probability of apprehension and punishment for reference group)
3. Social controls (number of evaders in reference group)
TAX EVASION BEHAVIOR
(unreported income, unwarranted deductions, failure to file a return)
1. Self-reports
2. Behavioral outcome measures
PSYCHOLOGICAL CONDITIONS
Personal instigations
1. Personal strain (estimates of difficulty meeting tax obligation and perceived unfairness of tax laws and authorities)
2. Personal orientation (self-serving versus community orientation)
Personal constraints
1. Perceived opportunity (subjective estimate of opportunity to evade)
2. Perceived risk of punishment (certainty and severity)
3. Intolerance of tax evasion (attitudes and moral beliefs about the propriety of evasion)

Figure 3-1
A Social Pychological Model of Tax Evasion Behavior

CHAPTER 4
RESEARCH DESIGN AND DATA ANALYSIS METHODS

Introduction

The research methods used to test the hypotheses that were developed in Chapter 3 are explained in this chapter. First, there is a description of the experiment used to measure the effects of procedural injustice on attitudes towards tax compliance and on tax reporting behavior. Second, the chapter includes a discussion of the methods used to analyze the data generated by the administration of the experiment.

Experimental Design

Overview

The experimental task required that subjects participate in an interactive, financial decision making simulation using a computer. Subjects were asked to assume the role of a small business owner with excess cash available to invest. As part of this role, subjects decided how to invest their small business income, and they decided how much small business and investment income to report on their tax returns. One hundred and forty-four members of various community groups in Gainesville, Florida participated in the study in exchange for cash rewards.

All three of the independent variables--inaccuracy, inconsistency and indirect inequity were manipulated between-subjects. Two dependent variables--compliance rate and compliance attitudes--and two covariates--psychological reactance and prior attitudes toward the IRS--were included in the study. The experimental task, the subjects, the incentive structure, the independent variables, the dependent variables, and the covariates will be discussed in turn.

Experimental Task

General setting. An attempt was made to conceal the true purpose of the experiment by adding irrelevant decisions to the experimental setting and by giving a vague explanation of the purpose of the experiment. Although the purpose of this study is to measure the effects of procedural injustice on attitudes towards tax evasion and tax reporting choices, subjects were also asked to make other financial decisions in addition to making tax reporting decisions. Tax reporting decisions were embedded within a computerized personal financial setting in which subjects were asked to invest earnings generated by a small, cash-only business. Subjects were led to believe that their decisions in this setting would help the researcher understand how individuals make personal financial decisions. The rationale for this obfuscation was twofold: first, to reduce the possibility of bias in subject's responses caused by demand effects to evade taxes (Webley et al. 1991), and second, to reduce any sensitivity that subjects might have about revealing information about themselves (Elffers et al. 1987).

The experimental setting was created using computer software rather than manually for several reasons. The computer was used in the experimental manipulations to better represent the taxing authority as a nameless, faceless bureaucracy incapable of recognizing or correcting its own mistakes. In addition, the use of a computer to implement the inaccuracy manipulation described later was particularly appropriate since the IRS uses computer-generated notices to audit investment income. Moreover, computers also reduced the computations required of subjects and eliminated the need to use dice or other mechanical devices to determine probablistic outcomes. Finally, the computer allowed the experimenter to have control over the order of information provided to the subjects.

Several provisions were made to facilitate the use of computers to administer the experiment. For example, the computer software contains error-checking routines that prompt the user to re-enter data that is nonsensical. Also, the software includes a practice session allowing subjects to become familiar with the tasks they will perform when the experiment begins. Finally, because the program does not permit users to go backward, written instructions were provided so that participants could refer to the instructions at any point in the experiment.

Each experimental session began with the administration of verbal instructions (see Appendix A for a copy of these instructions as well as a copy of the screens that subjects viewed during the experiment). Because most of the instructions subjects received were administered using interactive computer software, the verbal instructions included only brief instructions concerning the nature of the experiment and the use of the computer. After receiving verbal instructions, participants typed a code word to activate the computer program. Once activated, the software provided a brief overview of the nature and purpose of the experiment.[11] Then, subjects were led through a series of screens where they answered questions concerning their feelings about various statements.[12]

After answering these questions, participants were presented with a series of screens containing instructions for the experiment.[13] As indicated in the instructions, participants were asked to think of themselves as owners of a small business with excess cash to invest. Further, they were told they would receive two types of income: small business income and investment income. By having participants open one of ten envelopes they received before the experimental sessions began, small business income was assigned to participants during each of seven experimental periods, or years as they were referred to in the experiment.[14] The amount of small business income assigned varied from period to period within a range of eleven thousand to sixteen thousand dollars.[15] Unlike small business income, investment income had to be earned. Subjects read that they could earn investment income during each period by allocating cash available for investment at the beginning of each period to a portfolio of mutual funds.[16] Participants were not required to invest their available cash; however, cash not invested earned no return.[17]

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[11] This information was presented to participants in screens #1 through #4 in Appendix A.

[12] Some of these questions, found in screens #5 through #44 of Appendix A, were included to measure participant's attitudes towards the IRS and their level of psychological reactance.

[13] These instructions are found in screens #45 through #59 in Appendix A.

[14] Ten, rather than seven, envelopes were given to the subjects to minimize the possibility that subjects would change their behavior near the end of the experiment.

[15] The amount of small business income assigned each year was as follows: practice trial-$15,000, year 1-$13,300, year 2-$15,250, year 3-$11,050, year 4-$14,730, year 5-$12,470, year 6-$16125, and year 7-$14,320.

In addition to making decisions regarding how to invest their available cash, subjects were also asked to file a tax return at the end of each period. Each return had two lines: one to report business income and a second line to report investment income.[18] While completing the return, subjects decided how much investment income and small business earnings to report. After filing the tax return, subjects viewed a summary of their tax payments, investment income and small business income for the period that displayed how much cash remained after tax payments.[19] This cash amount was then carried over to the next period where the subject could choose to hold or invest it.

After the seventh period, subjects were presented with additional questions regarding their feelings towards investing and taxpaying.[20] In addition, subjects answered a series of debriefing questions. When these questions were completed, the program terminated and subjects were given a debriefing form as they exited the computer laboratory.

Investments. In the instructions for the experiment, subjects were asked to consider income from their small, cash-only businesses as "extra" cash available for investing because the salary income they earned from their regular job was sufficient to provide for their needs.

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[16] Subjects were asked to allocate their cash to their investment choices in screen #60 in Appendix A.

[17] For the first period, participants were given a cash endowment to invest. In later periods, cash available for investment consisted of the investment and small business income earned in prior periods less tax payments.

[18] Screen #65 in Appendix A contains an example of the tax return subjects completed.

[19] This financial summary is shown in screen #75 in Appendix A.

[20] These questions were presented in screens #95 through #136 in Appendix A.

At the same time they were given a schedule of five available investment opportunities consisting of a certificate of deposit and four different mutual funds.[21] During each experimental period, only two of the four mutual funds were offered as investment options, and these varied from one period to the next in order to avoid having a rote investment decision.

At the beginning of each period, subjects allocated their available cash (consisting of business income and investment income from prior periods less tax-related payments from prior periods) among the three investment options displayed on their screens.[22] In addition to investing, subjects were allowed to retain some or all of their available investment capital as cash. However, any amount retained as cash had a zero return. After allocating their cash to the available investment options, subjects viewed a screen that listed their investment returns for the period.[23]

The risk and return characteristics of the investments offered to subjects each period were obviously not patterned after similar real-world investment options since the probability of a loss was zero for all the investments. Rather, the risk and return characteristics of these hypothetical investments along with the zero return on cash were designed to insure that all participants would earn investment income in each of the seven periods. This was required for the inaccuracy manipulation, as explained below, to be successful.

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[21] The list of investment options is found on screen #48 in Appendix A.

[22] Screen #79 in Appendix A is an example of the screen subjects saw at this point in the experiment.

[23] Screen #80 in Appendix A is an example of what subjects viewed at this point.

Tax returns. Near the end of each experimental period, subjects were asked to declare the amount of small business and investment income they wished to report on their tax returns for that period.[24] Prior to doing this, subjects were asked to write down the investment income they had earned for the period, and they were instructed to open the envelope corresponding to the current year to discover how much small business income was earned for the year. Both types of income reported on the return were subject to a flat 28% tax rate. However, the two types of income were subject to different audit procedures in the experiment as they would be in a real world setting.[25]

In the experiment, participants were informed that the IRS would match the amount of investment income they reported each period with the investment income they earned as reported on information returns filed by the investment companies. This information was provided to lead participants to believe that the probability of audit and the probability of detection would be 100% for investment income. However, no actual audit of investment income was conducted during the experiment. Instead, when subjects reported less than the full amount of investment income for the period, the computer program generated a prompt reminding them that the audit rate on investment income was 100% and asking them if they would like to make any changes to the return. This encouraged subjects to always report 100% of their investment income, which was necessary in order for the inaccuracy manipulation described below to be effective.

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[24] Screen #83 in Appendix A is representative of the tax returns participants were asked to file each period.

[25] Through its Information Returns Program, the IRS uses computers to compare the amount of investment income taxpayers actually reported to the amount reported on information returns from banks and corporations. Therefore, the audit rate on most types of investment income is approximately 100%. The audit rate for small businesses in any given year is substantially less than 100%.

Unlike the investment income that is included in the experiment to conceal the purpose of the experiment and to provide a richer setting for tax reporting behavior, small business income was included so that changes in tax reporting behavior could be measured across experimental treatments. To allow tax reporting choices to vary, the audit rate on business income was set significantly lower than the audit rate for investment income. Subjects were informed that audits of small business income would be conducted randomly each period, and they were also told the probability of being selected for audit in a given period was one in ten. This audit rate approximates audit rates used in other lab experiments involving tax compliance (Dubin et al. 1990).[26] Finally, subjects were told that if they were audited, small business income not reported was certain to be detected.

Like investment income, the amount of small business income reported on the tax return was never audited during the experiment. This was done to control for any experimental bias that could have been introduced if subjects reacted to audits.

After subjects filed their tax returns for each period, each was informed he/she would not be audited that period. After receiving this message, each was asked to deliver the envelope containing the amount of their small business income for that period to a proctor stationed at a master computer terminal. As each subject observed the screen of the master computer, the proctor asked for and entered that subject's computer station number. Also, the proctor entered the period number and the small business income for the period, which was on a slip of paper, in the envelope he received. When this was accomplished, a password flashed on the screen which the subjects entered in their computers when they returned to their desks. Once the appropriate password was entered, a new screen appeared containing a summary of the income reported and the tax-related payments that were made for the period.[27]

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[26] In recent years, the actual federal audit rate has fallen to about 1 percent (Dubin, Graetz, and Wilde 1990).

In the instructions for the experiment, participants were told that this process was necessary so that their computers could update their cash balances for the period as well as conduct audits of small business income for those who had been selected. In actuality, the software each participant used contained the true small business income each subject was assigned for each of the seven periods. This deception was included to convince participant's that audits of small business income would be conducted randomly without regard to the amount of small business income reported.

In addition to the audit rate, participants were given the penalty rate for underreporting investment and small business income. For both types of income, the penalty for not reporting income was 50 percent of the amount of tax evaded.[28] In addition to payment of the penalty, participants were instructed that back taxes would have to be paid. Therefore, the actual expected value of not paying a dollar in tax on small business income was 85 cents, but the expected value of not paying a dollar in tax on investment income was a loss of 50 cents. The expected value of not paying a dollar of tax in this experiment is equal to (1-rp-r) where r is the audit rate and p is the dollar amount of the penalty. The difference in expected values is the result of using an audit rate of 100 percent for investment income as opposed to an audit rate of 10 percent for small business income. This difference in expected values was designed to be an incentive for subjects to report all of their investment income while simultaneously allowing them to vary their tax reporting choice for small business income in response to the experimental manipulations.

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[27] An example of this tax summary can be found at screen #91 in Appendix A.

[28] The actual maximum amount that can be levied for fraudulent reporting is a penalty of 75 percent of back taxes plus back taxes.

In addition to the penalty for underreporting, participants were informed of a second cost that would have to be paid in the event of a small business income audit. They were told that they would have to pay for the cost of responding to the audit even in those instances when the audit revealed that the correct amount of business income was reported. This cost was meant to operationalize the real world costs of responding to tax audits such as professional fees, lost time, and frustration. Subjects were instructed that these audit costs would, on average, be $1000 but could be either higher or lower than this amount in any given situation.

To make this audit cost salient to the subjects, an audit of small business income was conducted during the practice trial for all subjects. Subjects were charged an audit cost of $1000. This amount was subtracted as part of tax-related payments from small business and investment income thereby reducing the amount of cash held at the end of the practice trial.

As with investment income, the inclusion of this additional cost in the experiment was necessary in order to facilitate the inaccuracy and inconsistency manipulations described later. It reinforced the idea that responding to IRS notices and audits is costly even when the subject has done nothing wrong.

Subjects

Subjects who are experienced taxpayers are more representative of the population of taxpayers because their responses in experimental settings are conditioned by their prior attitudes concerning the tax system and their experiences within it (Hite 1988). Because this study involves measuring attitudes towards tax enforcement, it was essential to obtain participants who had formed these attitudes through their own experiences as taxpayers.

Participants for this study were recruited through various community groups in the Gainesville, Florida area.[29] The experiment was promoted, by personal contact and by advertising in the local paper, as a fund-raising opportunity whereby community groups could earn money by providing volunteers for a financial decision-making study. To help insure that most participants would be seasoned taxpayers, groups leaders were informed that only group members who were at least twenty-five years old would be permitted to participate.[30]

Incentive Structure

Community groups participating in the experiment were paid according to a two-tiered compensation scheme. First, community groups were paid a fixed cash bonus that was tied to the number of volunteers the group provided rather than the performance of the volunteers. Details of the bonus arrangement are presented in Table 4-1. This graduated bonus arrangement was designed to encourage community groups to provide large numbers of subjects in order to expeditiously complete the experiment.

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[29] In addition to providing the researcher with subjects having taxpaying experience, this approach proved to be a very efficient way of gathering subjects since most of the groups involved provided at least ten subjects with some groups providing over thirty subjects.

[30] Only fifteen out of one hundred and forty four subjects included in the analysis had never filed a tax return. The mean number of returns filed was approximately seventeen.

In addition to the cash bonuses, community groups were told they would receive additional cash rewards based on the performance of their volunteers during the study. Prior to participating, community group leaders were informed that the average for all individuals from the group would be approximately 15 dollars per person.[31] After all the sessions of the experiment were conducted, it was determined that none of the groups had earned the average of 15 dollars per person.[32] To maintain the goodwill of the groups, they were all paid fifteen dollars for each subject they provided after the experiment was completed. Individual subjects had no knowledge of this final payout during the experiment.

During the administration of the verbal instructions, subjects were told that the money they earned for their group would be determined by the amount of cash they had on hand at the conclusion of the exercise. The intent of these instructions was to remind participants that their decisions would affect the amount of the cash reward they earned for their group through the choices they made regarding where to invest and how much small business income to report on the tax returns. The experimental cash subjects held at the end of period seven was made up of the cash each subject received to begin the experiment[33] plus the investment and small business income earned over seven periods less the tax-related payments made over the same period of time. Tax-related payments consisted of income taxes, audit costs and underpayment penalties. However, since no actual audits of small business income were conducted, only subjects in the inaccuracy treatment group paid audit costs and underpayment penalties.

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[31] This amount assumes that subjects would obtain an after-tax return on investments over all seven periods equal to the average expected value of the mutual funds available during the experiment. Also, it assumes that subjects would underreport approximately half of their small business income.

[32] The actual average payout per person was approximately $12.86.

[33] For subjects not receiving the inaccuracy treatment, this amount was $33,000. Those who were in the inaccuracy treatment group received $33,955.

To assist participants in understanding how their decisions affected the compensation they earned for their group, they viewed two screens at the end of each period. First, participants saw a simple cash reconciliation summarizing the effects of their decisions for that period. Next, they viewed two graphs that showed their total earnings and current period earnings in terms of experimental and real dollars.[34]

This financial incentive, unlike the cash bonuses, was presented to individual subjects for two reasons. First, since the task required cognitive effort, the incentive was included to encourage subjects to pay attention to the task. Second, the incentive was introduced to operationalize the economic incentive to underreport small business income in the laboratory setting.

Operationalization of Independent Variables

Inaccuracy. The manipulation of inaccuracy was between subjects and took place after three periods with four periods remaining. Those subjects who received the inaccuracy treatment were requested to pay additional taxes and penalties on their third period investment income in spite of the fact that they truthfully reported this income. This demand for payment came in the form of a semi-official IRS letter with the subject's name and address printed at the top of the letter.[35] The letter stated that the subject failed to report $2000 of investment income earned in the third period and requested an additional payment of $840. Further, the letter explained that the $840 payment requested was the sum of additional taxes of $560 and $260 in penalties.

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[34] The two screens referred to here are screen #92 and screen #93 in Appendix A.

[35] Subjects receiving the inaccuracy treatment viewed screens #86 through #90 in Appendix A. Earlier in the experiment, the subjects entered a fictitious name and address which the computer used to generate the letter.

After viewing the letter, subjects were informed that they could respond in one of two ways. They could pay the additional tax and penalty or contest the assessment. The subjects were told that contesting the notice would involve hiring professionals to resolve the problem that would cost, on average, $300, but could cost more or less than this. If subjects decided to contest, they were charged $300 dollars in professional fees, and they were informed that the professional that they had hired to resolve the matter was unsuccessful in convincing the IRS that its claims were erroneous. Further, they were informed that the IRS had levied their bank accounts for the additional tax and penalties requested in the notice. If subjects did not contest, their cash balances were reduced by the amount of tax and penalty the IRS claimed they owed.[36]

Inconsistency. Inconsistency was also manipulated between subjects. Participants were randomly assigned to computer stations and informed that they were in the blue group or the red group. Near each of the twenty-six computers in the lab, large colored signs labeled either "blue group" or "red group" were hung. Half of the computer stations had red signs placed near them with the other half receiving blue signs. During the administration of the verbal instructions, subjects were told that these signs indicated their group membership and that their responses would be organized according to their group membership and subject numbers.

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[36] No matter how the subjects responded to the inaccurate IRS penalty notice, the effect of the notice was to reduce the experimental cash they held at the end of the experiment and their actual cash payments. To equalize the expected value of compensation across treatments groups, those who received the inaccuracy treatment had their initial cash endowments increased by 955 dollars which represents the approximate present value of the costs incurred by these subjects if they contested the IRS claims.

Near the end of the practice trial, subjects were given the audit rates on small business income. Regardless of the group to which they were assigned, those receiving the inconsistency treatment were told that the audit rate on small business income for their group would be one in ten, while the audit rate for the other group would be one in thirty. The wide disparity in audit rates across the two groups was chosen purposely to strengthen the experimental manipulation. Those subjects who did not receive the inconsistency treatment were simply informed that they would face an audit rate of one in ten on the small business income they report.[37]

Indirect inequity. As with inconsistency and inaccuracy, indirect inequity was treated as a between subjects treatment. This variable was manipulated while subjects were reading the instructions presented to them on their computer screens concerning the conversion of cash earned in the experiment to real cash.

Those who received the manipulation were shown a table that they were told summarized the computation of the payouts for participants in a prior research session. The table depicted the conversion of experimental dollars into real dollars and the amounts actually paid. In a column labeled "Amount Paid," half of the cash payments listed were substantially less than the other half. An asterisk was placed by these low cash payments, and another asterisk was placed near the bottom of the screen with an explanatory note adjacent to it. The note explained that these individuals had been charged with additional taxes and penalties for not reporting all of their small business income because of an IRS error.[38]

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[37] Screens #72 and #73 in Appendix A are indicative of the screens those in the inconsistency treatment viewed.

Those subjects who did not receive the indirect inequity manipulation, viewed a similar table during the administration of the compensation instructions; however, the "amount paid" column showed that all previous participants had been paid the correct amount of cash compensation.

Measurement of Dependent Variables

The hypotheses in the previous chapter describe the effects of procedural injustice on tax reporting choices and on attitudes towards tax compliance. These hypotheses state that procedural injustices caused by inconsistencies in tax enforcement and by tax enforcement based upon inaccurate information will cause a change in taxpayers' attitudes so that they are more accepting of tax cheating. Further, the hypotheses state that these forms of procedural injustice will also cause taxpayers to be less compliant. Finally, one set of the hypotheses posits that these affects are also produced when procedural injustice is experienced indirectly. The measurement of compliance attitudes will be discussed first followed by the measurement of compliance rates.

Compliance attitudes. Attitudes towards tax compliance were measured using the 15-item composite noncompliance scale that was used in a large scale tax compliance survey conducted by Yankelovich, Skelly and White (1984).[39] The scale was designed to measure individual feelings toward the acceptability of tax cheating. Subjects responded to these questions through the computer immediately after the experimental periods were completed so that the differential effects of the experimental manipulations on subjects' attitudes could be measured. To avoid sensitizing subjects to the true nature of the experiment, questions related to investment philosophy were interspersed among the questions that measured compliance attitudes.

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[38] This screen is found in Appendix A labeled as screen #57.

[39] The questions that make up this attitudinal scale are located at screens #96, #98, #99, #102, #103, #105, #107, #108, #110, #112, #113, #115, #116, #117, and #119.

Compliance rate. In the experimental tax compliance literature, several different measures of tax reporting behavior have been employed (Alm et al. 1992, Elffers et al. 1992, Webley et al. 1991). Several of these approaches to measuring this dependent variable were used in this study to insure that the results obtained were not sensitive to the way in which tax reporting behavior was measured.

One measure was the ratio of reported small business income to actual small business income over the last four periods.[40] The second measure of tax compliance was the total amount of small business income reported over the last four periods. The third measure, frequency of noncompliance, was the number of instances during the last four periods when less that the full amount of small business income was reported. Finally, the last measure, occurrence of noncompliance, was dichotomous. It had a value of zero if there were no instances of noncompliance during the last four periods and a value of one otherwise.

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[40] This variable was transformed in an attempt to normalize its distribution and to stabilize its variance using the transformation Winer (1971) recommends for proportions data. For values not close to one or zero the transformation is: xnew=2 arc sin (sqrt(xold)). For values close to one or zero, the transformation is: xnew= 2 arc sin (sqrt(xold plus or minus 1/2n)) where n equals the number of observations. For this formula, 1/2n is added when the value is close to 0 and subtracted when the value is close to one.

The dependent variables described above were computed using small business income reported during the last four experimental periods. Although subjects made tax reporting decisions in all seven of the experimental periods, only the four periods that occurred subsequent to the inaccuracy manipulation were of interest.[41]

Measurement of Covariates

Psychological reactance. While the procedural justice manipulations were predicted to decrease the compliance rate of those exposed to procedural inequity, the level of psychological reactance within each subjects may have supplemented, mitigated or even reversed this prediction. To control for this individual difference variable, a measure of psychological reactance was included as a covariate in the model. The level of psychological reactance among subjects was measured at the beginning of the experiment using Merz's psychological reactance scale (1983).[42]

Prior attitudes towards the IRS. Taxpayers bring with them to experimental settings patterns of behavior and attitudes that have evolved over time. In particular, most experienced taxpayers have likely formed attitudes towards the IRS either as the result of their own experiences with the IRS or from hearing of other's experiences. To control for the possibility that differing attitudes toward the IRS may have affected individual responses to the experimental manipulations, a second covariate representing a measure of prior attitudes toward the IRS was also included in the model.

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[41] The indirect inequity and consistency manipulations occurred at the beginning of the experiment; therefore, behavior in the first three as well as in the last four periods should have been affected by these manipulations. Accordingly, the analyses summarized in Table 5-3 in Chapter 5 were repeated with taxpaying behavior measured over all seven periods of the experiment. The results obtained were similar to those obtained using a four year period except that indirect inequity was no longer significant when taxpaying behavior was measured dichotomously. It was, however, still significant when the number of years of underreporting was used to measure taxpaying behavior.

[42] The questions that make up the reactance scale are found in screens #6, #7, #8, #11, #12, #13, #16, #17, #20, #21, #22, #23, #24, #27, #28, #30, #31, #32, #34, #35, #36, #37, #39, #40, #41, #42, #43, and #44 in Appendix A.

These attitudes about the honesty and fairness of the IRS were measured at the beginning of the experiment using the composite attitude scale developed by Yankelovich, Skelly and White (1984). These questions were mingled with other questions regarding participant's attitudes toward investing to avoid disclosing the real intent of the experiment.[43]

Data Analysis Methods

Model

Both actual compliance rate and compliance attitudes were modeled as functions of three procedural justice variables and two individual difference variables. The first two procedural justice variables, inaccuracy and inconsistency, are constructs for procedural injustice that is personally felt, while the third procedural justice variable, indirect inequity, is a construct for procedural injustice that is experienced vicariously. Two covariates, prior attitudes toward the IRS and psychological reactance, were included in the model to control for individual differences. Because these variables were the only variables of interest, all other variables (e.g., tax rate, penalty rate) that might otherwise be expected to affect attitudes and tax reporting choices were held constant across all factor levels in the experiment.

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[43] The questions measuring attitudes toward the IRS are found in screens #9, #14, #18, and #25 in Appendix A.

Analysis

Data from the experiment was analyzed using ANCOVA with a 2 X 2 X 2 factorial design treating inconsistency, inaccuracy and indirect inequity as between subjects factors. A graphical representation of this design is found in Figure 4-1. The continuous variables representing psychological reactance and prior attitudes toward the IRS were included in the analysis as covariates in order to control for the effects of individual differences.

To supplement the ANCOVA analysis, two other analyses were performed. ANCOVA assumes that dependent variables are continuous and normally distributed. Because most subjects did not underreport small business income in any of the last four periods, the distributions of all four measures of taxpaying behavior were skewed, a violation of the normality assumption.[44] Further, two measures of taxpaying behavior, the frequency of noncompliance and the occurrence of noncompliance, were discrete variables rather than continuous. For example, the variable measuring the occurrence of noncompliance was given a value of one if there had been at least one instance of underreporting and zero otherwise. Similarly, the variable measuring the frequency of noncompliance was a count variable that represented the number of times during the last four periods of the experiment small business income was underreported.

The relationship between the dependent variable representing the occurrence of noncompliance and the independent variables inaccuracy, inconsistency, indirect inequity, psychological reactance, and prior attitudes toward the IRS was analyzed using logistic regression. Logistic regression explicitly assumes that dependent variables are binary. Specifically, it assumes that the probability of obtaining a zero or one outcome is described by the logistic distribution:

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[44] Only twenty-five of the one hundred forty-four subjects underreported small business income at least once during the last four periods of the experiment.

$$Prob\ (Y=1)=\frac{e^{B'x}}{1+e^{B'x}}$$

In logistic regression, this probability function is used to construct a likelihood function that is maximized to obtain estimates of coefficients and their standard errors. Once these estimates have been obtained, significance testing is completed by comparing coefficient estimates with the size of their standard errors.

When used to analyze the occurrence of noncompliance, logistic regression analysis supplements ANCOVA in two respects. First, logistic regression assumes the error term for the model is distributed according to a binomial distribution rather than a normal distribution (Hosmer and Lemeshow 1989). Therefore, the results obtained from logistic regression will not be biased by the skewness observed in the data; rather, the skewness of the data is incorporated into the model. Second, logistic regression corrects for the heteroscedastic error term variance encountered when using ANCOVA to analyze binary dependent variables, thereby increasing the efficiency of the parameter estimates (Greene 1993).

To study the relationship between the frequency of noncompliance and the procedural justice variables and covariates, the Poisson regression model was used in addition to ANCOVA. Unlike ANCOVA, Poisson regression accounts for the large number of zero values obtained for the frequency of noncompliance and for the discrete nature of this dependent variable (Greene 1993). The Poisson regression model was designed to analyze count data such as the number of times an event occurs within a given period of time. According to this model, each observation of the dependent variable y_i comes from a Poisson distribution illustrated by the equation

$$Prob(Y_i = y_i) = \frac{e^{-\lambda_i}\lambda_i^{y_i}}{y_i!}, \quad y_i = 0,1,2,...$$

The parameter λ_i from the Poisson distribution is assumed to be related to the independent variables x_i by the equation

$$\ln\lambda_i = \mathbf{B}'\mathbf{x}_i.$$

To estimate the coefficients for the independent variables and their standard errors, a likelihood function constructed using the Poisson distribution is maximized. Once obtained, these values are used to test the significance of the independent variables in the model the same way they are used in a linear regression model.

When analyzing count data like the frequency of noncompliance in this study, Poisson regression can supplement ANCOVA because the model specifically accounts for the characteristics of count data such as non-normality and heteroscedasticity. When counts are large, the normal distribution will approximate the actual discrete distribution of the dependent variable. However, when counts are small, as they are for the frequency of noncompliance in this study, the dependent variable will not be normally distributed and, consequently, OLS regression will be inappropriate (Lovett and Flowerdew 1989). Because the Poisson regression model assumes that dependent variables are distributed according to the discrete Poisson distribution rather than the normal distribution, it was used to examine the sensitivity of the ANCOVA model to the skewness of the count data in this study. Finally, the Poisson regression model was used in addition to ANCOVA in this study because it does not assume that the data is homoscedastic like the ANCOVA model. Because count data is inherently heteroscedastic, this approach should produce more efficient parameter estimates.

Summary

The experiment described in this chapter includes several features designed to test the research hypotheses. During the experiment, experienced subjects make tax reporting choices in the context of a making personal financial decisions. Subjects are exposed to manipulations designed to produce feelings of procedural injustice resulting from inconsistency, inaccuracy and indirect inequity. To encourage a high level of involvement, organizations to which the subjects belong were paid according to the decisions made by subjects during the experiment. To analyze the data from the experiment, ANCOVA is used because it accounts for individual difference variables, psychological reactance and prior attitudes about the IRS, to statistically increase the power of the analysis and to determine if these variable have a significant affect on the dependent variables. Also, logistic regression and Poisson regression are used to augment ANCOVA for two of the four measures of taxpaying behavior.

Table 4-1
Bonus Compensation

Number of Subjects Provided	Incremental Bonus	Total Bonus
Between 0 and 9	$0	$0
Between 10 and 19	$150	$150
Between 20 and 29	$200	$350
30 and Above	$250	$600

Indirect Inequity

	Consistent	Inconsistent
Accurate		
Inaccurate		

No Indirect Inequity

	Consistent	Inconsistent
Accurate		
Inaccurate		

Figure 4-1
Experimental Design

CHAPTER 5
RESEARCH RESULTS

Introduction

This chapter contains the results of the tests of the hypotheses described in Chapter 3. Chapter 5 begins with a discussion of the descriptive data obtained on the subjects who participated in the study and on the dependent variables analyzed. Next, the results obtained for each of the three procedural justice variables described in Chapter 3 are examined. Following this discussion, subject responses to various debriefing questions designed to measure the effectiveness of the experimental design are analyzed. Finally, this chapter concludes with a discussion and analysis of the results.

Descriptive Data

Demographic Data

Subjects were obtained by placing an advertisement in the local newspaper asking for community groups to participate in a financial decision-making research study. Community groups such as parent teacher associations, amateur athletic organizations and civic clubs provided the 144 subjects who participated in the experiment. Because a wide variety of community groups elected to participate, the subject pool was diverse in terms of the gender, age, income level and tax experience of participants. This demographic information is summarized in Table 5-1. When answering demographic questions, subjects were given the option of not responding to a question. Therefore, Tables 5-1 and 2 indicate how many subjects did not respond to each question.

As indicated in Table 5-1, close to an equal number of men and women participated in the experiment. The mean age of participants was approximately 40 years of age. Considering only those participants responding to the age question, the number of participants above the mean was 48 while the number below the mean was 47. This symmetry of the distribution of subject's ages is reflected in the frequencies listed in Table 5-1.

Participants were concentrated in the higher income levels. The majority of the subjects reported annual income of over $30,000 per year (73.6%). However, as reflected in Table 5-1, 9.7% of the subjects reported earnings of less than $15,000 per year and 14% reported earning between $15,000 and $30,000 per year.

On average, the subjects in the sample were experienced-tax filers. The mean number of tax returns filed per subject was approximately 17. Of the 141 subjects responding, only 15 reported that they had never filed a tax return while 78 reported that they had filed more than 15 tax returns.

Finally, as reported in Table 5-1, the mean response to a question asking subjects to estimate their marginal tax rates was approximately 18 percent. Admittedly, the subjects participating in this experiment were not drawn from a random sample of taxpayers. Nevertheless, with the possible exception of income level, the demographic data enumerated above do not indicate that there is a systematic bias in the subject pool which would bias the research findings described below.

Dependent Variables

Several measures of tax reporting behavior were used to assess the effect of the independent variables on taxpaying choices. The first of these measures was total small business income not reported (TNOTRBI). It consisted of the sum of small business income that was not reported during the last four periods of the experiment. The second measure was related to the first in that it was total small business income not reported as a proportion of the total small business income earned during the last four periods of the experiment (ASPROP).[45] The third measure used was a binary compliance variable (BICOMP) which had a value of either '0' or '1' depending upon each subject's compliance behavior. If a subject underreported small business income during any of the last four periods of the experiment, then that subject received a score of '1' for the BICOMP variable. On the other hand, subjects who did not underreport small business income during the last four periods received a score of '0'. Finally, the last measure of tax reporting behavior was the number of years or periods the subject underreported small business income (NOYEARS). The periods included in the count were periods four through seven. Accordingly, values for this measure varied from 0 to 4 with the higher scores representing more frequent underreporting of small business income.

In addition to measuring tax reporting behavior, compliance attitudes (CA) were measured to determine if the independent variables studied would affect attitudes as well as behavior. As explained in the previous chapter, these attitudes were measured using the attitudinal scale developed by Yankelovich, Skelly and White (1984). By construction of this scale, the lower a subject's score for the variable CA, the greater their agreement with the proposition that tax cheating is acceptable.

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[45] As indicated in footnote number 30 in Chapter 4, this variable was transformed using an arc sine transformation in an attempt to stabilize the variances.

The most striking result found in a descriptive analysis of the dependent variables was that only 25 out of 144 subjects (17.4%) underreported small business income at least once during the last four periods of the experiment. This result was obtained in spite of the relatively high expected value available to subjects for failing to report one dollar of small business income. Given the combination of penalties and audit rates that subjects were told they could expect during the experiment, the expected value of not reporting one dollar of business income was 85 cents.

This result--that most of the participants in the study did not underreport small business income--caused the distributions for the dependent variables measuring tax reporting behavior to be highly skewed. This is clearly evident in the frequency histograms of these variables found in Figures 5-1 through 5-4. For each figure, the intervals along the X axis should be interpreted as including amounts greater than the number listed below the prior interval and less than or equal to the number listed under the interval itself. For example, the second interval along the X axis of Figure 5-1 represents total small business income not reported of greater than zero and less than or equal to $5000. In addition, the numbers found above the bars of the frequency histograms represent the number of subjects with values of the dependent variable falling within the indicated intervals. Finally, the first interval along the X axis of the frequency histograms in Figures 5-1 through 5-4 does not represent an interval of values at all. Rather it represents the value obtained when subjects did not underreport small business income at any time during the last four periods of the experiment.[46]

Other than the fact that the tax reporting choice variables are skewed, it is also evident from Figures 5-1 and 5-4 that the decision to underreport business income was not an all or nothing proposition for subjects. If subjects did not report any small business income during the last four periods of the experiment, TNOTRBI would have been equal to $57,645. As indicated in Figure 5-1, only 2 of the 25 subjects did not report any income in any of the last four periods. In fact, the majority of the subjects who underreported business income during the experiment underreported $20,000 or less during the last four periods. The same conclusions can be drawn from Figure 5-4. Only 9 of 25 subjects evading taxes underreported business income during all of the last four experimental periods. For the remainder, the decision to evade taxes in one period did not necessarily lead to the same decision in subsequent periods.

Unlike the tax reporting choice variables, the distribution of compliance attitudes (CA) was not severely skewed. As seen in the frequency histogram of CA in Figure 5-5, the distribution is fairly symmetric with the exception of the upper tail which is larger than the lower tail. Since higher values of CA were indicative of greater attitudinal compliance, a concentration of subjects in the upper intervals of the histogram indicates that subject's behavior was correlated with their attitudes--subjects who did not underreport business income tended to also score high on the attitudinal scale. Although, the residual plot for CA in Figure 5-10 is more representative of a variable that is normally distributed than residual plots of the tax reporting choice variables, the Bowman and Shenton statistic in Table 5-2 indicates that CA, like the tax reporting choice variables, is not normally distributed.

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[46] For the variables TNOTRBI, BICOMP and NOYEARS this value is zero. However, the arc sin transformation used on ASPROP resulted in a value of approximately .12.

To determine the extent to which the skewness in the dependent variables measuring tax reporting choice would affect the distribution of error terms in an ANCOVA model, four separate ANCOVA analyses were performed, one for each of the four tax reporting choice variables above, using consistency (CONSIST), indirect inequity (INDIRECT), accuracy (ACCURATE), reactance (REACT), and prior attitudes toward the IRS (PIRSATT) as independent variables. The residuals from these models were then plotted against the fitted values for the dependent variables. The resulting graphs are found in Figures 5-6 through 5-9. The two parallel lines running through the graphs are plus or minus 2 standard deviations from the mean of the error terms. It is obvious from viewing Figures 5-6 through 5-9 that the error terms from ANCOVA models using TNOTRBI, ASPROP, BICOMP, and NOYEARS as dependent variables are not normally distributed. This conclusion was confirmed by computing the Bowman and Shenton chi-squared statistic to test the null hypothesis that the tax reporting choice variables were normally distributed. This statistic is distributed as a chi-square distribution with 2 degrees of freedom and is computed as follows:

$$L = n\,[\text{skewness}^2 / 6 + (\text{kurtosis} - 3)^2 / 24]$$

The statistics and p values for the tests of normality in Table 5-2 clearly indicate that TNOTRBI, ASPROP, BICOMP, and NOYEARS are not normally distributed.

One of the assumptions of the ANCOVA model is that the error terms from the model are normally distributed. This assumption is clearly not satisfied when using the tax reporting choice variables as dependent variables. Nevertheless, ANCOVA was used to test the hypothesized relationships between consistency, indirect inequity, and accuracy and tax reporting behavior because ANCOVA is robust to violations of the normality assumption when the number of subject in each cell of the experiment are equal (Glass et al. 1972). Also, for the reasons described in Chapter 4, the ANCOVA analysis of BICOMP and NOYEARS was augmented with additional analyses which recognized the discrete nature of these two variables. BICOMP was analyzed using logistic regression and NOYEARS was analyzed using Poisson regression. Because the logistic regression and Poisson regression models do not assume the data is normally distributed, the results from these models will not be affected by the skewness of the tax reporting choice variables.

Hypotheses Tests

Four separate ANCOVA analyses were performed to test the hypotheses stated in Chapter 3 regarding the effect of consistency, accuracy and indirect inequity on tax reporting behavior. In other words, one ANCOVA analysis was performed for each of the four tax reporting choice variables TNOTRBI, ASPROP, BICOMP, and NOYEARS. Because the results obtained were sensitive to the dependent variable used to measure tax reporting behavior and to the form of analysis employed, the discussion below refers to the results obtained in all of the ANCOVA analyses and supplemental analyses.

Table 5-3 includes a summary of all of the results obtained for both the tax reporting choice variables and for compliance attitudes. The first column lists the independent variables CONSIST, ACCURATE and INDIRECT which were hypothesized to affect tax reporting behaviors and attitudes. Also listed are the covariates REACT and PIRSATT and the interaction term CONSIST X ACCURATE. Since no other interaction terms were significant in the models, they are not found in the table. The first four columns in the table summarize the results of the four ANCOVA models described above. Column titles indicate the dependent variable used in the analysis. The fifth and six columns list the results of the logistic regression analysis on BICOMP and of the Poisson regression on NOYEARS. Finally, the last column contains the results of the ANCOVA analysis performed on CA to determine how consistency, accuracy and indirect inequity affect compliance attitudes. As indicated in the notes to Table 5-3, the data in each column of the table is supported by ancillary tables and graphs. For each ANCOVA analysis, there is included an ANCOVA table, a table of mean comparisons by factor level, and a graph of variable cell means.

Consistency

Before discussing the effects of consistency on the tax reporting behavior variables and on compliance attitudes, the hypotheses from Chapter 3 regarding consistency are restated:

> H1: The failure to enforce tax laws consistently across taxpayers will make taxpayers more acceptant of noncompliance with the tax laws.
>
> H2: The failure to enforce tax laws consistently across taxpayers will increase the level of noncompliance with the tax laws.

Essentially, the hypotheses above predict that inconsistent audit rates as manipulated in the experiment would lead to greater rates of underreporting small business income and to more negative attitudes regarding the propriety of tax evasion. In reality, the opposite effect was observed. As presented in Table 5-3, the p values reported for the variable CONSIST indicate that this variable is highly significant without regard to the dependent variable used to measure tax reporting choices, the form of analysis employed, or whether the dependent variable is measuring attitudes or behavior. Mean comparisons by factor levels in Tables 5-7, 5-9, 5-11, and 5-13 all show that means of the dependent variables measuring reporting behavior are much lower when audit rates are inconsistent than when they are consistent. Moreover, the graphs of the cell means for the same variables in Figures 5-11 through 5-14 portray the same result. Finally, the signs on the coefficients for the variable CONSIST in both the logistic regression analysis of BICOMP and the Poisson regression using NOYEARS as the dependent variable found in Tables 5-16 and 5-17 are both negative indicating that inconsistent audit rates are decreasing the level of noncompliance.

Compliance attitudes, like behavior, were also significantly affected by audit inconsistency as presented in the last column of Table 5-3. The direction of the effect can be seen in Table 5-15 and in Figure 5-15. Because the variable CA becomes smaller as the propensity of subjects to agree that tax evasion is acceptable increases, some care is required in interpreting the aforementioned table and figure. For example, the mean value of CA is lower when audit rates were consistent which means that subjects with consistent audit rates, on average, were more inclined to agree that tax evasion was acceptable when answering the questions from the attitudinal scale. Thus, the effect of applying inconsistent audit rates in the experiment was to increase subject's attitudinal propensity to comply with the tax laws.

Perhaps a partial explanation for this unexpected result can be found in subject's responses to the two manipulation checks in Appendix B designed to test subjects understanding of and feelings toward the inconsistency manipulation. Question number six in Appendix B asked subjects to indicate whether their audit rate was higher than, lower than, or the same as the audit rate for other participants. As indicated in Appendix B, 56.3% of the subjects responding to the question correctly answered that their audit rate was higher. Question number seven in Appendix B asked subjects to rate the fairness of the comparative audit rates using a five point Likert scale. The results reported in Appendix B indicate that there was no strong consensus among the subjects regarding the fairness of the inconsistent audit rates.[47] In fact, approximately the same number of subjects rated the arrangement as very fair as rated the arrangement very unfair.

It is difficult to deduce why there was no strong feeling among subjects that inconsistent audit rates were unfair. One explanation is that they failed to understand that their audit rates were ostensibly higher. A second possibility is that although subject's perceived their audit rates were higher, they did not feel this was unfair since they were never financially penalized as the result of an audit of small business income. Finally, subjects may have felt that inconsistent audit rates were fair regardless of whether such rates resulted in an audit.

Although the inconsistency manipulation apparently failed to generate substantial feelings of procedural injustice, it did produced a strong deterrence effect. This effect may have been caused by juxtaposing the subject's audit rate with the supposed lower audit rate of other participants. Perhaps framing audit rates in this manner increased individual's subjective audit rates thus increasing the deterrence effect of the audit rate beyond that of those not receiving the inconsistency manipulation.

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[47] An additional ANCOVA was performed using only those subjects who were exposed to the inconsistent group. In addition to reactance and prior attitudes toward the IRS, subjects responses to question 7 in Appendix B was included as a covariate to determine if subjects' fairness perceptions were significantly correlated with their behavior. The results of this analysis show that fairness perceptions about inconsistent audit rates were not significantly related to the taxpaying behavior of subjects who received the inconsistent audit rates.

Accuracy

In addition to consistency, accuracy was hypothesized to be a component of procedural justice which, when lacking, would lead to greater noncompliance and would negatively affect attitudes towards taxpaying. The formal hypotheses relating to consistency are restated below:

> H3: IRS enforcement actions based upon inaccurate information will make taxpayers more acceptant of noncompliance with the tax laws.
>
> H4: IRS enforcement actions based upon inaccurate information will increase the level of noncompliance with the tax laws.

Unlike consistency, accuracy affected taxpaying behavior in the hypothesized direction. However, hypothesis three above could only be confirmed for some of the measures of tax reporting behavior. When using TNOTRBI as the dependent variable, ACCURATE was moderately significant with a p value of .1049. Mean comparisons of the factor levels for accuracy in Table 5-7 and the graph of the cell means in Figure 5-11 show that the effect of accuracy on the amount of business income underreported was in the hypothesized direction. That is, the inaccuracy manipulation increased the mean business income not reported for the treatment group.

In addition to the variable ACCURATE, the interaction term CONSIST X ACCURATE had a significance level of .0856 when TNOTRBI was used as the dependent variable. However, this interaction was not significant in any of the other analyses performed. Figure 5-11 displays the nature of the interaction between the variables CONSIST and ACCURATE. The first two bars show the effect of the accuracy treatment when audit rates are consistent and indirect inequity is present. The third and fourth bars show the effect of accuracy treatment given that audit rates are inconsistent and indirect inequity is present. Clearly, inconsistent audit rates severely attenuated the effect of the accuracy treatment on the amount of business income underreported. As described earlier, inconsistent audit rates, probably because of the way they were viewed by subjects, produced a strong deterrence effect. This effect may explain the observed interaction between consistency and accuracy.

The variable ACCURATE was more significant when NOYEARS was used as the dependent variable in the ANCOVA analysis and as a dependent variable in the Poisson regression. For the ANCOVA analysis of NOYEARS, the p value of ACCURATE was .0367, and for the Poisson regression, the p value of ACCURATE was .0022. Again, mean comparisons of factor levels for accuracy in Table 5-13 and the graph of cell means in Figure 5-14 indicate that the group of subjects who received the letter from the IRS in the third period of the experiment underreported business income for more periods, on average, than the subjects who did not receive the letter. Finally, the sign on the coefficient for the variable ACCURATE found in the Poisson regression results in Table 5-17 was positive also indicating a positive relationship between inaccuracy and the number of periods that subject's underreported business income.

When ASPROP and BICOMP were used as measures for tax reporting behavior, accuracy was not statistically significant in explaining tax reporting behavior as seen in Table 5-3. Although the relationship between accuracy and these variables was not significant, comparisons of factor level means and cell means for the variables ASPROP and BICOMP in Tables 5-9 and 5-11 and in Figures 5-12 and 5-13 show that the accuracy manipulation did increase the mean level of noncompliance for the treatment group both in dollars and in terms of the number of noncompliers.

Although accuracy did significantly affect some measures of tax reporting behavior, it did not significantly affect compliance attitudes as indicated in Table 5-3. However, the accuracy manipulation did affect attitudes in the direction hypothesized. The group receiving the accuracy manipulation had lower average scores on the compliance attitudinal scale indicating greater agreement with the acceptability of tax evasion. This pattern is observed both in the comparison of factor means in Table 5-15 and in the graph of cell means in Figure 5-15.

To determine how subjects receiving the accuracy treatment perceived it, two questions, reproduced in Appendix B as questions eight and nine, were asked as manipulation checks near the conclusion of the experiment. The first question subjects answered was whether the letter they had received from the IRS was correct or incorrect. Of those responding to the question, 98.6% understood that the letter was incorrect. Further, when subjects were asked to rate the fairness of having to make additional payments to the IRS as a result of receiving the letter, 77.1% responded that this situation was very unfair. This response was on the extreme end of a five point Likert scale. Apparently, most of the subjects who received the letter understood that it was incorrect and felt that it resulted in unfair treatment by the IRS.

Indirect Inequity

Audit consistency and accuracy were hypothesized to affect tax compliance behavior and attitudes through taxpayers' direct, personal experiences with procedural injustice. Similarly, experiencing procedural injustice vicariously by learning of someone else's negative experiences with tax enforcement authorities was also hypothesized to affect tax reporting behavior and compliance. These hypotheses, formally stated in Chapter 3, are restated below:

> H5: Taxpayers experiencing IRS procedural injustice indirectly will become more acceptant of noncompliance with the tax laws.
>
> H6: Taxpayers experiencing IRS procedural injustice indirectly will increase their level of noncompliance with the tax law.

As with the accuracy variable, hypothesis five above can only be confirmed with a subset of the dependent variables used to measure compliance behavior. The variable INDIRECT is significantly related to the two discrete measures of compliance behavior, BICOMP and NOYEARS. The p values for INDIRECT in the ANCOVA analysis of BICOMP and NOYEARS, as reported in Table 5-3, are .0737 and .0298, respectively. Moreover, the logistic regression analysis of BICOMP and the Poisson regression using NOYEARS as the dependent variable yield similar results. As displayed in Table 5-3, the variable INDIRECT has a significance level of .0585 in the logistic regression analysis of BICOMP and a significance level of .0007 in the Poisson regression on NOYEARS. However, when the continuous variables TNOTRBI and ASPROP are used as dependent variables in the ANCOVA analysis, the variable INDIRECT is not significant.

Although INDIRECT was not significant for all measures of compliance behavior, its effect was in the hypothesized direction regardless of the measure of taxpaying behavior used. That is, the subject group receiving the indirect injustice manipulation was less compliant both in terms of dollars of business income not reported and number of years of noncompliance. Mean comparisons found in Tables 5-7, 5-9, 5-11, and 5-13 as well as graphs of cell means in Figures 5-11 through 5-14 show this assertion to be valid. In addition, signs of the estimated coefficients for the variable INDIRECT in the logistic regression analysis in Table 5-16 and in the Poisson regression in Table 5-17 are positive indicating a positive correlation between indirect procedural injustice and subject's decisions to underreport business income as well as the number of years of underreporting business income.

Subjects receiving the indirect injustice manipulation were asked to respond to two questions to determine if the manipulation was successful. In the first question, they were asked whether previous participants earned as much as they should have.[48] Of those subjects responding to the question, 57.1% answered that prior participants had not been paid as much as they should have been paid. The second question asked subjects to rate how fairly prior participants were compensated using a five point Likert scale. The mean response to this question was 3.329. These results show that there was no consensus among subjects that prior subjects had not received the correct compensation or, as a result, that prior subjects had been treated unfairly.[49] These results must be interpreted with caution, however. Several times during the administration of the experiment subjects indicated that the two questions above were ambiguous because they were unsure whether the questions referred to the overall compensation scheme and payoffs or to the individual experiences of the participants listed in the payoff table. Consequently, the results obtained from the manipulation checks for the indirect injustice manipulation may be a manifestation of poorly designed manipulation check questions rather than a failure to successful manipulate indirect injustice.

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[48] These questions are questions ten and eleven in Appendix B.

[49] An additional ANCOVA was performed using only those subjects who were exposed to the indirect inequity treatment. In addition to reactance and prior attitudes toward the IRS, subjects responses to question 11 in Appendix B was included as a covariate to determine if subjects' fairness perceptions were significantly correlated with their behavior. The results of this analysis show that fairness perceptions about indirect inequity were not significantly related to the taxpaying behavior of subjects who received the indirect inequity treatment.

Covariates

Two covariates, a measure of prior attitudes toward the IRS (PIRSATT) and a measure of reactance (REACT), were included in the statistical analyses performed to control for individual differences among the subjects. Although no formal hypotheses regarding the effect of the covariates on compliance behavior and attitudes were stated in Chapter 3, it was expected that negative attitudes toward the IRS and high levels of reactance would be accompanied by high levels of noncompliance and negative attitudes towards tax compliance.

The effect of REACT on taxpaying behavior varied according to the dependent variable used to measure behavior. The variable REACT was significantly related to tax reporting behavior only when the discrete tax compliance variables BICOMP and NOYEARS were analyzed. As shown in Table 5-3, REACT attained a significance level of .0334 when BICOMP was analyzed using ANCOVA, and it attained a significance level of .0252 when logistic regression was applied to BICOMP. Further, REACT had a significance level of .0152 in the ANCOVA analysis on NOYEARS and a significance level of .0001 in the Poisson regression on NOYEARS. As expected, the signs on the coefficients for REACT in Tables 5-10, 5-12, 5-16 and 5-17 were all positive indicating a positive correlation between the variable REACT and the decision to underreport business income and the number of years business income was underreported. Although REACT was significantly related to some measures of taxpaying behavior, it was not significantly related to compliance attitudes as measured by the variable CA.

The covariate PIRSATT was significant in an even narrower range of circumstances than REACT. PIRSATT was marginally significant only in the logistic regression analysis of BICOMP with a p value of .0986 For the logistic regression analysis in Table 5-16, the coefficient on PIRSATT was positive indicating a positive correlation between negative attitudes toward the IRS and the decision to underreport business income as well as the number of years business income was underreported. Like the covariate REACT, PIRSATT was not significantly related to compliance attitudes.

Supplemental Analysis

In addition to the analyses described above, two supplemental analyses of the effects of the procedural justice variables on taxpaying behavior were conducted. First, a MANCOVA was performed on the measures of taxpaying behavior using the same independent variables as in the ANCOVA analysis. Second, an additional covariate, taxpaying behavior in the first three periods of the experiment, was included along with reactance and prior attitudes toward the IRS in an additional ANCOVA analysis. The results from these analyses indicate that both accuracy and indirect inequity may not be as significant in affecting taxpaying behavior as indicated previously.

When multiple measures of the same construct are used in separate ANCOVA analyses, it is possible that the overall probability of a Type I error will be magnified beyond the single probability of a Type I error for each ANCOVA. To control for this possibility, a MANCOVA was performed using the dependent variables TNOTRBI, ASPROP, BICOMP and NOYEARS. The results of this analysis are displayed in Table 5-4. These results show than only CONSIST and CONSIST*ACCURATE are significant when MANCOVA rather than ANCOVA is used to determine how consistency, accuracy and indirect inequity affect taxpaying behavior. As before, inconsistent audit rates produced significantly greater levels of compliance.

The are two possible explanations for why indirect inequity and accuracy are no longer significant when MANCOVA is used. First, the overall Type I error rate may have indeed been magnified by using multiple measures of taxpaying behavior. Second, MANCOVA may have underestimated the significance of indirect inequity and accuracy since all the measures of taxpaying behavior obtained in this study were highly skewed and some were discrete which are violations of the multivariate normality assumption of MANCOVA. Whatever the explanation for these results, the prior results for accuracy and indirect inequity should be viewed with some caution.

Unlike the manipulations of consistency and indirect inequity which occurred at the beginning of the experiment, accuracy was manipulated at the end of the third period. To determine if subject's taxpaying behavior changed after receiving the accuracy manipulation, a measure of their behavior during the first three periods of the experiment was included as a covariate in the ANCOVA model described earlier. The results of this analysis on the variable ACCURATE are displayed in Table 5-5. The high degree of significance found on the covariates measuring taxpaying behavior during the first three periods of the experiment indicates that subjects' behavior in the first three periods of the experiment was highly correlated with their behavior during the last four periods. More importantly, accuracy is no longer significantly related to any of the measures of taxpaying behavior. Therefore, the accuracy manipulation did not produce a significant change in taxpaying behavior in spite of the fact tax cheating was greater among subjects who received the accuracy manipulation than among subjects who did not. This disparity indicates that subjects assigned to the accuracy treatment may have had a higher initial propensity to evade taxes than those assigned to the control group even though they were randomly assigned.

Debriefing Information

The experiment was designed as a small business and investing simulation in order to involve subjects and to conceal the nature of the experiment. The responses to several debriefing questions found in Appendix B shed light on whether these objectives were achieved. The first of these questions asked subjects to rate how interesting they found the experiment to be. A majority of the subjects (88.2%), found the exercise to be either interesting or very interesting. In a related question, subjects were asked to rate the realism of the experiment using a five point Likert scale with the value '1' representing "very unrealistic" and the value '5' representing "very realistic". The mean response to this question (3.514) and the distribution of the responses indicate that, generally, subjects found the exercise to be realistic.

Another question was asked to determine if subjects had discovered the true purpose of the experiment. Subjects were given several blank lines on their computer screens to record their responses. Approximately half of the subjects stated that the experiment was about tax reporting and investing behavior. However, only 13.2% of the subjects answered that the experiment was about tax reporting behavior alone, and only two subjects correctly guessed that the experiment was testing the effect of procedural injustice on tax reporting. Although the majority of subjects generally felt that tax reporting was part of the experiment, demand effects should not have been great given that so few of them understood what the experiment was actually about.

Other questions designed to elicit subject's feelings as to how their behavior in the experiment might compare with their behavior in a similar real world setting were included with the other debriefing questions. In response to a question asking if they would make the same taxpaying decisions in the real world that they had made in the experiment, 77.1% of the subjects responded that they would make the same decisions. This magnitude of this response and the fact that the majority of participants did not underreport business income, regardless of the financial incentives to do so, implies that the experiment succeeded in producing actual taxpaying behavior--at least for a large number of the subjects. The 30 subjects who responded that they would behave differently were given a follow-up question asking them to describe in their own words how they would behave differently. Only 11 of the thirty replied that they would report more income on their own tax returns. The remainder either did not respond or replied that they would change some other aspect of their behavior.

Because several of the community groups providing participants for the experiment were involved in experimental sessions held on different days, there was a concern that members of the groups with knowledge of the experiment would discuss the experiment with other members who would participate in future sessions. To avoid this problem, subjects were given a debriefing handout at the end of the experiment which asked them not to discuss the experiment with anyone for a period of time. To determine if subjects knew anything about the experiment beforehand, they were asked if they had previously discussed the experiment with anyone. Of the subjects responding to the question, all indicated that they had not.

Discussion of the Results

When one views Table 5-3 several broad trends are evident. The first is that the variable CONSIST, no matter how taxpaying behavior is measured or analyzed, is significantly related to taxpaying behavior and compliance attitudes. However, the effects of inconsistent audit rates were not as expected since they caused subjects to report more business income and to be more in agreement with the propriety of tax compliance. The previous discussion on consistency offered an explanation as to why these effects were observed.

The second pattern observed in Table 5-3 is that, with the exception of PIRSATT, the significance of the independent variables included in the models of tax reporting behavior using the discrete variables BICOMP and NOYEARS as dependent variables did not vary according to the form of analysis used. For example, the variables CONSIST, INDIRECT, and REACT were significant in the ANCOVA analysis of BICOMP. The same variables were significant in the logistic regression analysis of BICOMP. Similarly, the variables CONSIST, INDIRECT, ACCURATE and REACT were significant in the ANCOVA analysis of NOYEARS. Again, the same variables were significant in the Poisson regression on NOYEARS. Since logistic regression and Poisson regression are not based on the assumption that the dependent variables are normally distributed, it appears that the results obtained using BICOMP and NOYEARS to measure tax reporting behavior are not sensitive to the skewness observed in these variables.

Another trend observed in Table 5-3 is that the significance level of the variables INDIRECT, ACCURATE, and REACT increases when the variable measuring tax compliance behavior is changed from a continuous measure to a discrete measure. Further, this same pattern is observed when switching from the discrete variable, BICOMP, to NOYEARS. That is, the significance level of the variables INDIRECT, ACCURATE, and REACT is higher when NOYEARS is used as the dependent variable. This sensitivity of the results to the measure of tax compliance can be explained by observing in Figure 5-1 that many of the subjects who chose not to report business income underreported substantially less than the total small business income earned over the last four periods which was $57,645. In reality, many of them underreported small amounts of business income, and they did so in each of the last four periods of the experiment. Consequently, while their behavior did not have a great effect on the variables TNOTRBI and ASPROP, their behavior nevertheless affected the variables BICOMP and NOYEARS. Finally, the higher significance levels obtained when using NOYEARS as opposed to BICOMP were caused by the fact that NOYEARS is a finer measure of tax reporting behavior. The higher significance levels reflect the fact that the experimental manipulations had a stronger effect on the decision to be a habitual noncomplier than on the decision to either comply or not comply.

Finally, the last trend observable in Table 5-3 is that none of the independent variables, other than CONSIST, had any effect upon compliance attitudes. There are two possible explanations for this result. First, subjects may have behaved opportunistically when making taxpaying choices yet claimed to have had more ethical beliefs when responding to questions from the attitudinal scale. Prior research examining the relationship between taxpayer attitudes and actual taxpaying behavior has shown that taxpayers in real taxpaying situations behave similarly (Elffers et al. 1987). Second the attitudinal scale used to measure compliance attitudes may not have been measuring all of the attitudes relevant to the tax compliance decisions made during the experiment.

When the effects of accuracy and indirect inequity on taxpaying behavior are examined using MANCOVA and using behavior during the first three periods of the experiment as a covariate in ANCOVA, a different picture emerges than the one portrayed in Table 5-3. The MANCOVA results in Table 5-4 suggest that only consistency is significantly related to taxpaying behavior. However, it may not be appropriate to use MANCOVA given the distributional characteristics of the dependent variables examined in this study. Also, as indicated in Table 5-5, controlling for differences between the subject group receiving the accuracy treatment and the control group causes accuracy to no longer be significantly related to taxpaying behavior.

Summary

Although sensitive to the approach used to measure taxpaying behavior, the predictions for the effect of accuracy and indirect injustice on taxpaying behavior were supported using ANCOVA, logistic regression and poisson regression. However, accuracy and indirect injustice were not significantly related to compliance attitudes. Further, when MANCOVA was used to analyze the relationship between the procedural justice variables and taxpaying behavior, accuracy and indirect inequity were no longer significant. Finally, when subjects' initial propensity to engage in tax evasion was controlled by adding an additional covariate to the ANCOVA model, accuracy was no longer significant.

Consistency was a highly significant variable in all of the models tested; yet the predictions concerning the effects of consistency were not supported because consistency was significant in the opposite direction. That is, inconsistent audit rates caused taxpayers to be more rather than less compliant. Finally, the covariate measuring reactance was significantly related to taxpaying behavior for some measures of taxpaying behavior. The contributions and implications of these results are discussed in Chapter 6.

Table 5-1
Summary of Subject Demographic Information

Grouping Variable	Number	Percentage
Gender		
Male	72	50.0%
Female	70	47.9%
No response	2	2.1%
Age		
25-30	20	13.9%
31-35	14	9.7%
36-40	24	16.7%
41-45	24	16.7%
46-50	11	7.6%
51-55	6	4.2%
over 55	8	5.5%
No response	37	25.7%
Income Level		
0-15 thousand	14	9.7%
15-30 thousand	21	14.0%
30-60 thousand	58	40.3%
over 60 thousand	48	33.3%
No response	3	2.1%

Table 5-1 (continued)
Summary of Subject Demographic Information

Grouping Variable	Number	Percentage
Number of tax returns filed		
None	15	10.4%
1-5	14	9.7%
6-10	15	10.4%
11-15	19	13.2%
16-20	25	17.4%
21-25	30	20.8%
26-30	11	7.7%
over 30	12	8.3%
No response	3	2.1%

Table 5-1 (continued)
Summary of Subject Demographic Information

Grouping Variable	Mean	Standard Deviation
Age in years [a]	40.104	9.658
Number of tax returns filed[b]	17.269	11.129
Estimate of marginal tax rate[b]	18.014	13.408

[a] Thirty-eight observations for this variable were missing.

[b] Three observations for this variable were missing.

Table 5-2
Descriptive Statistics for the Dependent Variables

Dependent Variable[a]	Mean	Standard Deviation	Bowman / Shenton Statistic	p-value for B/S Statistic
TNOTRBI	3093.4	9837.3	1924.08750	0.000
ASPROP	.29432	.50066	1361.82880	0.000
BICOMP	.17361	.38010	76.08963	0.000
NOYEARS	.47222	1.1401	211.47209	0.000
CA	68.736	14.599	12.72624	0.002

[a] TNOTRBI = Total business income not reported over last four periods
ASPROP = Total business income not reported as a proportion of total business income
BICOMP = Binary variable equal to 1 if tax was evaded or equal to 0 otherwise
NOYEARS = Number of periods tax was evaded during last four periods
CA = Compliance attitudes measured using an attitudinal scale

Table 5-3
Significance Levels and Direction of Effects for the Independent Variables

Independent Variable	TNOTRBI[a]	ASPROP[b]	BICOMP[c]	NOYEARS[d]	BICOMP[e]	NOYEARS[f]	CA[g]
CONSIST	(-) /.0009	(-) /.0005	(-) /.0046	(-) /.0197	(-) /.0047	(-) /.0002	(+) /.0312
ACCURATE	(+) /.1049	(+) /.1185	(+) /.1550	(+) /.0367	(+) /.1793	(+) /.0022	(-) /.4161
INDIRECT	(+) /.3024	(+) /.2514	(+) /.0737	(+).0298	(+) /.0585	(+) /.0007	(-) /.2797
CONSIST X ACCURATE	.0856	.1139	.9246	.2474	na	na	.2402
REACT	(+) /.2153	(+) /.1363	(+) /.0334	(+) /.0152	(+) /.0252	(+) /.0001	(-) .1767
PIRSATT	(+) /.9303	(+) /.7514	(+) /.1470	(+)/.3414	(+) /.0986	(+) /.1576	(-) .4005

Note: The table above includes p values for all combinations of dependent and independent variables. The signs listed before the p values indicate how the manipulated variables and the covariates affected the dependent variables. The signs were as expected except for the those on consistency which were opposite to what was predicted.

[a] The ANCOVA table for TNOTRBI is located at Table 5-6, mean comparisons by factor levels are located at Table 5-7 and cell means are graphed in Figure 5-11.

[b] The ANCOVA table for ASPROP is located at Table 5-8, mean comparisons by factor levels are located at Table 5-9 and cell means are graphed in Figure 5-12.

[c] The ANCOVA table for BICOMP is located at Table 5-10, mean comparisons by factor levels are located at Table 5-11 and cell means are graphed in Figure 5-13.

[d] The ANCOVA table for NOYEARS is located at Table 5-12, mean comparisons by factor levels are located at Table 5-13 and cell means are graphed in Figure 5-14.

[e] The summary results from the logistic regression analysis on BICOMP are reported in this column. Table 5-16 contains the entire results of the logistic regression analysis on BICOMP.

[f] The summary results from the Poisson regression analysis on NOYEARS are reported in the column. Table 5-17 contains the entire results of the Poisson regression analysis on NOYEARS.

[g] The ANCOVA table for CA is located at Table 5-14, mean comparisons by factor levels are located at Table 5-15 and cell means are graphed in Figure 5-15.

Table 5-4
MANCOVA Results for All Measures of Taxpaying Behavior

Independent Variables[a]	DF	Wilk's Lambda	F	Pr > F
CONSIST	4	.8987	3.69	.0070
ACCURATE	4	.9546	1.56	.1895
INDIRECT	4	.9579	1.44	.2251
CONSIST*ACCURATE	4	.9345	2.29	.0627
REACT	4	.9475	1.81	.1299
PIRSATT	4	.9727	.92	.4544

Note: This table only reports values for Wilk's Lamda because the F statistics using Pillai's Trace, the Hotelling-Lawley Trace and Roy's Greatest Root are the same as those obtained using Wilk's Lambda..

[a]CONSIST = Consistency
INDIRECT = Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

Table 5-5
Significance Levels for ACCURATE when Behavior in First Three Periods is a Covariate

Independent Variable	TNOTRBI[a] 4-7	ASPROP[a] 4-7	BICOMP[a] 4-7	NOYEARS[a] 4-7	BICOMP[b] 4-7	NOYEARS[c] 4-7
ACCURATE	.8802	.7146	.3900	.9879	.2554	.5726
TNOTRBI 1-3	.0001	n/a	n/a	n/a	n/a	n/a
ASPROP 1-3	n/a	.0001	n/a	n/a	n/a	n/a
BICOMP 1-3	n/a	n/a	.0001	n/a	.0000	n/a
NOYEARS 1-3	n/a	n/a	n/a	.0001	n/a	.0000

[a] The relationship between accuracy and this measure of behavior for the last four periods of the experiment was analyzed with ANCOVA using the same measure of behavior for the first three periods as a covariate.

[b] The relationship between accuracy and this measure of behavior for the last four periods of the experiment was analyzed with logistic regression using the same measure of behavior for the first three periods as a covariate.

[c] The relationship between accuracy and this measure of behavior for the last four periods of the experiment was analyzed with Poisson regression using the same measure of behavior for the first three periods as a covariate.

Table 5-6
Dependent Variable is TNOTRBI

Panel A: Ancova Results

R^2=.1344

Sources[a]	DF	SS	F	Pr > F
CONSIST	1	1031629528	11.54	.0009
ACCURATE	1	238232692	2.66	.1049
INDIRECT	1	95835049	1.07	.3024
CONSIST*ACCURATE	1	268164670	3.00	.0856
INDIRECT*ACCURATE	1	4134108	.05	.8301
CONSIST*INDIRECT	1	82895498	.93	.3373
CONSIST*INDIRECT*ACCURATE	1	595938	.01	.9350
REACT	1	138594786	1.55	.2153
PIRSATT	1	685984	.01	.9303
Error	134	11979191852		
Panel B: Estimated Coefficients				
REACT		117.703		
PIRSATT		19.925		

Note: TNOTRBI represents the total amount of business income not reported during the last four periods of the experiment.

[a]CONSIST = Consistency
INDIRECT = Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

Table 5-7
TNOTRBI Mean Comparisons by Factor Levels

Factor[a]	Level	Mean
CONSIST	Consistent	5812.098
	Inconsistent	374.722
ACCURATE	Accurate	1880.542
	Inaccurate	4306.279
INDIRECT	Indirect equity	2339.074
	Indirect inequity	3847.747

Note: This table shows the means of the three experimental factors at each of the factor levels for the dependent variable TNOTRBI. TNOTRBI represents the total amount of business income not reported during the last four periods of the experiment. N=72 for each factor level.

[a] CONSIST = Consistency
INDIRECT = Indirect inequity
ACCURATE = Accuracy

Table 5-8
Dependent Variable is ASPROP

Panel A: Ancova Results

R^2=.143183

Sources[a]	DF	SS	F	Pr > F
CONSIST	1	2.9041	12.67	.0005
ACCURATE	1	.5656	2.47	.1185
INDIRECT	1	.3041	1.33	.2514
CONSIST*ACCURATE	1	.5804	2.53	.1139
INDIRECT*ACCURATE	1	.0316	.14	.7108
CONSIST*INDIRECT	1	.1941	.85	.3591
CONSIST*INDIRECT*ACCURATE	1	.0047	.02	.8861
REACT	1	.5147	2.25	.1363
PIRSATT	1	.0231	.10	.7514
Error	134	30.7126		
Panel B: Estimated Coefficients				
REACT		0.007		
PIRSATT		0.004		

Note: ASPROP represents the total amount of business income not reported during the last four periods of the experiment as a proportion of the total business income earned during the same time period.

[a]CONSIST = Consistency
INDIRECT = Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

Table 5-9
ASPROP Mean Comparisons by Factor Levels

Factor[a]	Level	Mean
CONSIST	Consistent	.439
	Inconsistent	.150
ACCURATE	Accurate	.237
	Inaccurate	.352
INDIRECT	Indirect equity	.253
	Indirect inequity	.336

Note: This table shows the means of the three experimental factors at each of the factor levels for the dependent variable ASPROP. ASPROP represents the total amount of business income not reported during the last four periods of the experiment as a proportion of the total business income earned during the same time period. N=72 for each factor level.

[a] CONSIST = Consistency
INDIRECT = Indirect inequity
ACCURATE = Accuracy

Table 5-10
Dependent Variable is BICOMP

Panel A: Ancova Results

R^2=.1461

Sources[a]	DF	SS	F	Pr > F
CONSIST	1	1.0941	8.31	.0046
ACCURATE	1	.2693	2.05	.1550
INDIRECT	1	.4278	3.25	.0737
CONSIST*ACCURATE	1	.0012	0.01	.9246
INDIRECT*ACCURATE	1	.2130	1.62	.2056
CONSIST*INDIRECT	1	.0002	0.00	.9697
CONSIST*INDIRECT*ACCURATE	1	.0007	0.01	.9406
REACT	1	.6085	4.62	.0334
PIRSATT	1	.2802	2.13	.1470
Error	134	17.6422		
Panel B: Estimated Coefficients				
REACT		.008		
PIRSATT		.013		

Note: BICOMP is a binary variable that equals one if, at any time during the last four periods of the experiment, the subject failed to report all business income. Otherwise, the value of the variable is zero.

[a]CONSIST = Consistency
INDIRECT= Indirect Injustice
ACCURATE = Accuracy
REACT= Reactance
PIRSATT = Prior attitudes towards the IRS

Table 5-11
BICOMP Mean Comparisons by Factor Levels

Factor[a]	Level	Mean
CONSIST	Consistent	.264
	Inconsistent	.083
ACCURATE	Accurate	.139
	Inaccurate	.208
INDIRECT	Indirect equity	.125
	Indirect inequity	.222

Note: This table shows the means of the three experimental factors at each of the factor levels for the dependent variable BICOMP. BICOMP is a binary variable in that it equals one if, at any time during the last four periods of the experiment, the subject failed to report all business income. Otherwise, the value of the variable is zero. Accordingly, the numbers above also represent the proportion of participants who failed to report some business income during at least one period. N=72 for each factor level.

[a]CONSIST = Consistency
INDIRECT = Indirect inequity
ACCURATE = Accuracy

Table 5-12
Dependent Variable is NOYEARS

Panel A: Ancova Results R^2=.1596				
Sources[a]	DF	SS	F	Pr > F
CONSIST	1	6.4926	5.57	.0197
ACCURATE	1	5.1921	4.45	.0367
INDIRECT	1	5.6240	4.82	.0298
CONSIST*ACCURATE	1	1.5736	1.35	.2474
INDIRECT*ACCURATE	1	2.2995	1.97	.1625
CONSIST*INDIRECT	1	.1214	.10	.7474
CONSIST*INDIRECT*ACCURATE	1	.1117	.10	.7573
REACT	1	7.0512	6.05	.0152
PIRSATT	1	1.0630	.91	.3414
Error	134	185.8888		
Panel B: Estimated Coefficients				
REACT		.026		
PIRSATT		.025		

Note: NOYEARS represents the number of periods during the last four periods of the experiment in which the subject failed to report all of the business income received.

[a]CONSIST = Consistency
INDIRECT= Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

Table 5-13
NOYEARS Mean Comparisons by Factor Levels

Factor[a]	Level	Mean
CONSIST	Consistent	.694
	Inconsistent	.135
ACCURATE	Accurate	.306
	Inaccurate	.639
INDIRECT	Indirect equity	.458
	Indirect inequity	.540

Note: This table shows the means of the three experimental factors at each of the factor levels for the dependent variable NOYEARS. NOYEARS represents the number of periods during the last four periods of the experiment in which the subject failed to report all of the business income received. N=72 for each factor level.

[a] CONSIST = Consistency
INDIRECT = Indirect inequity
ACCURATE = Accuracy

Table 5-14
Dependent Variable is CA

Panel A: Ancova Results
R^2=.0759

Sources[a]	DF	SS	F	Pr > F
CONSIST	1	996.3037	4.74	.0312
ACCURATE	1	139.8318	.67	.4161
INDIRECT	1	247.5944	1.18	.2797
CONSIST*ACCURATE	1	292.5328	1.39	.2402
INDIRECT*ACCURATE	1	3.8893	.02	.8920
CONSIST*INDIRECT	1	71.1406	.34	.5617
CONSIST*INDIRECT*ACCURATE	1	5.9756	.03	.8663
REACT	1	387.7166	1.84	.1767
PIRSATT	1	149.4956	.71	.4005
Error	134	28160.8091		
Panel B: Estimated Coefficients[b]				
REACT		-.197		
PIRSATT		-.294		

Note: CA represents compliance attitudes as measured by the attitudinal scale used in the experiment.

[a] CONSIST = Consistency
INDIRECT = Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

[b] The scale used to measure compliance attitudes was structured to yield lower composite scores as the attitudinal propensity for noncompliance increased. Hence, the coefficients listed here are negative.

Table 5-15
CA Mean Comparisons by Factor Levels

Factor[a]	Level	Mean
CONSIST	Consistent	66.028
	Inconsistent	71.444
ACCURATE	Accurate	69.514
	Inaccurate	67.958
INDIRECT	Indirect equity	69.903
	Indirect inequity	67.569

Note: This table shows the means of the three experimental factors at each of the factor levels for the dependent variable CA. CA represents compliance attitudes as measured by the attitudinal scale used in the experiment. The scale was structured to yield lower composite scores as the attitudinal propensity for noncompliance increased. Hence, lower means are indicative of greater attitudinal acceptance of noncompliance. N=72 for each factor level.

[a] CONSIST = Consistency
INDIRECT = Indirect inequity
ACCURATE = Accuracy

Table 5-16
Logistic Regression Maximum Likelihood Estimates for BICOMP

Logistic Regression Results

Variable[a]	Coefficient	Std. Error	t-ratio	Prob |t| ≥ x
CONSIST	-1.5258	.5401	-2.825	0.00473
ACCURATE	0.6549	.4876	1.343	0.17934
INDIRECT	0.9492	.5016	1.892	0.05845
REACT	0.0643	.0287	2.238	0.02520
PIRSATT	0.1131	.0649	1.652	0.09856

Note: No interaction terms were included in the logistic regression analysis of BICOMP since none of the interactions terms in the ANCOVA analysis of BICOMP were significant.

[a]CONSIST = Consistency
INDIRECT = Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

Table 5-17
Poisson Regression for NOYEARS

Poisson Regression

Variable[a]	Coefficient	Std. Error	t-ratio	Prob |t| ≥ x
CONSIST	-1.0119	.2753	-3.676	.00024
ACCURATE	0.8093	.2638	3.067	.00216
INDIRECT	0.8999	.2645	3.403	.00067
REACT	0.0581	.0149	3.898	.00010
PIRSATT	0.0429	.0342	1.413	.15762

Note: No interaction terms were included in the Poisson regression since none of the interaction terms in the ANCOVA analysis of NOYEARS were significant.

[a]CONSIST = Consistency
INDIRECT = Indirect Injustice
ACCURATE = Accuracy
REACT = Reactance
PIRSATT = Prior attitudes towards the IRS

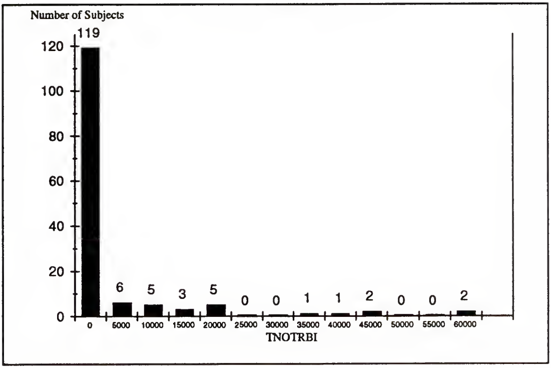


Figure 5-1
TNOTRBI Frequency Histogram

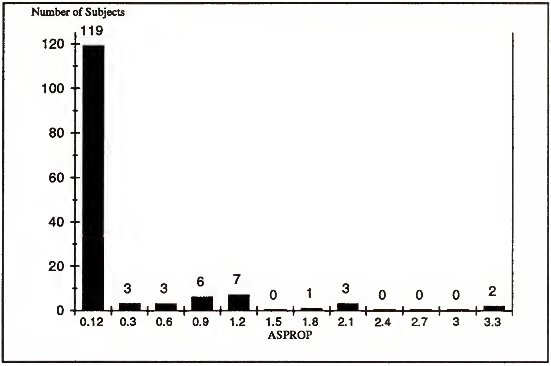


Figure 5-2
ASPROP Frequency Histogram

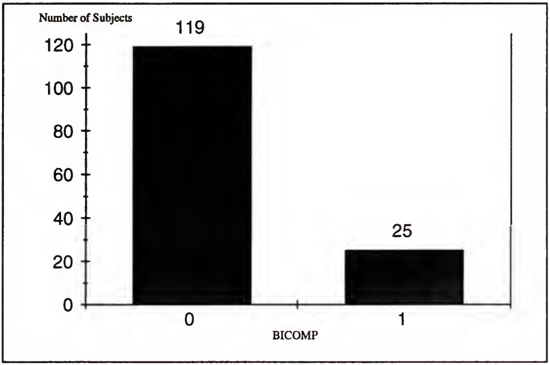


Figure 5-3
BICOMP Frequency Histogram

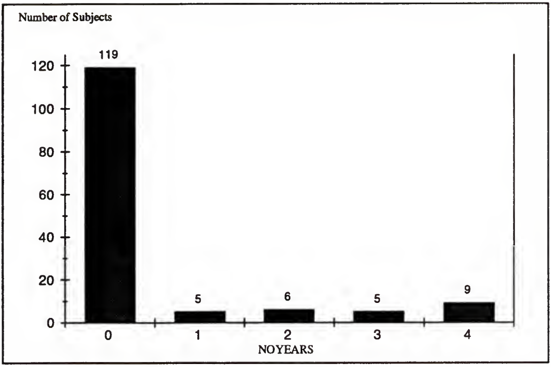


Figure 5-4
NOYEARS Frequency Histogram

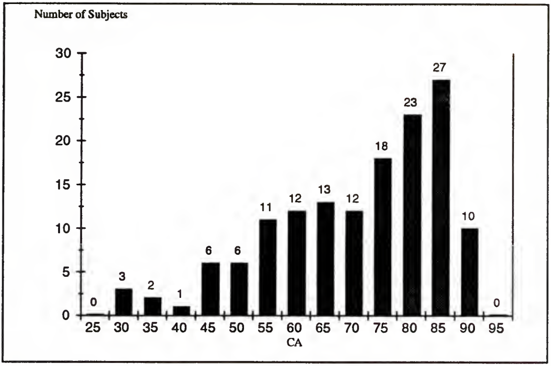


Figure 5-5
CA Frequency Histogram

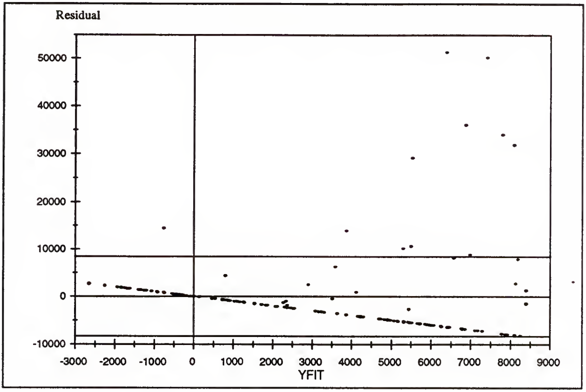


Figure 5-6
Residual Plot
Dependent Variable is TNOTRBI

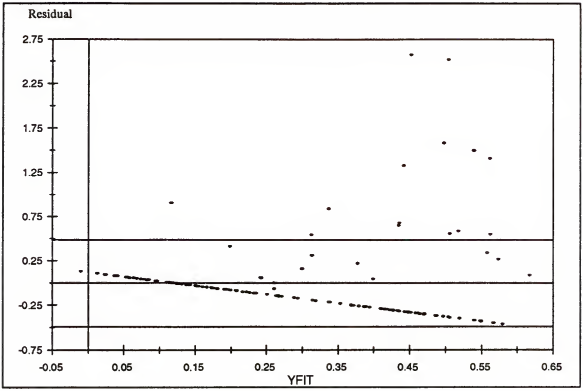


Figure 5-7
Residual Plot
Dependent Variable is ASPROP

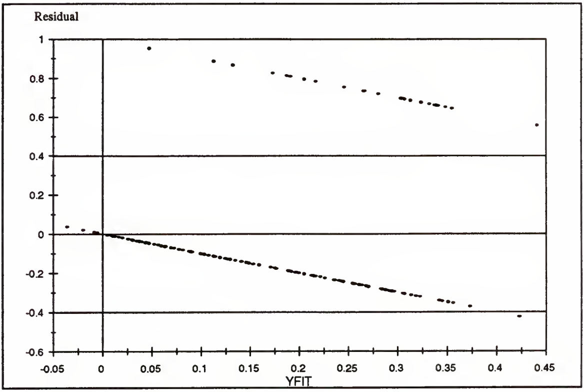


Figure 5-8
Residual Plot
Dependent Variable is BICOMP

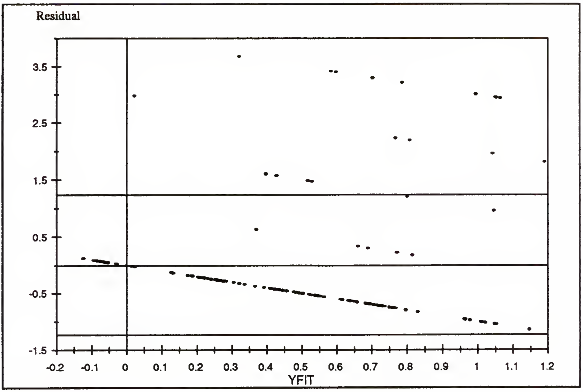


Figure 5-9
Residual Plot
Dependent Variable is NOYEARS

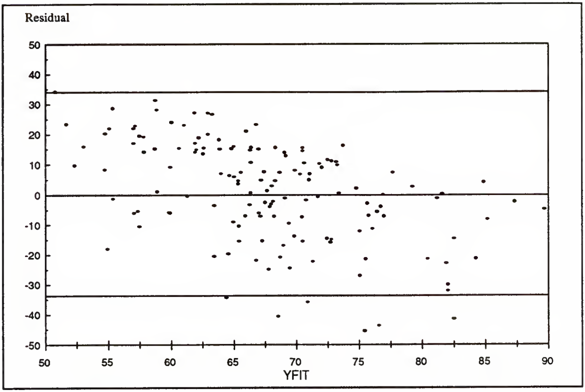


Figure 5-10
Residual Plot
Dependent Variable is CA

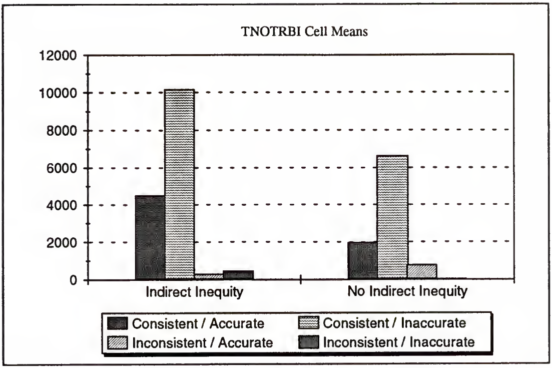


Figure 5-11
TNOTRBI Cell Means

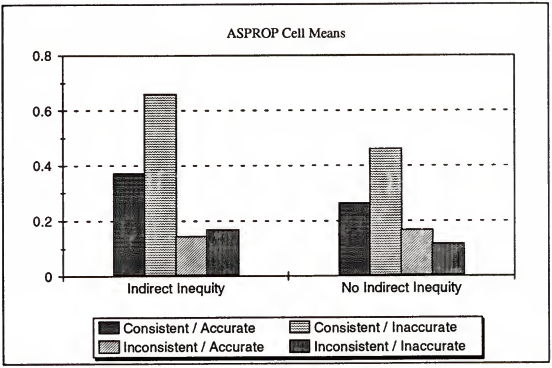


Figure 5-12
ASPROP Cell Means

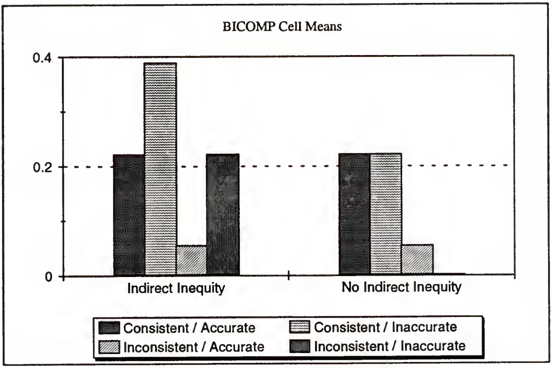


Figure 5-13
BICOMP Cell Means

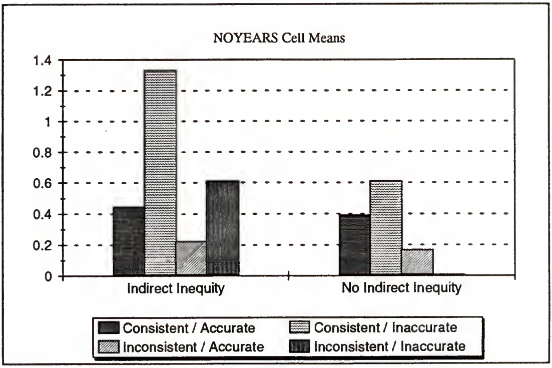


Figure 5-14
NOYEARS Cell Means

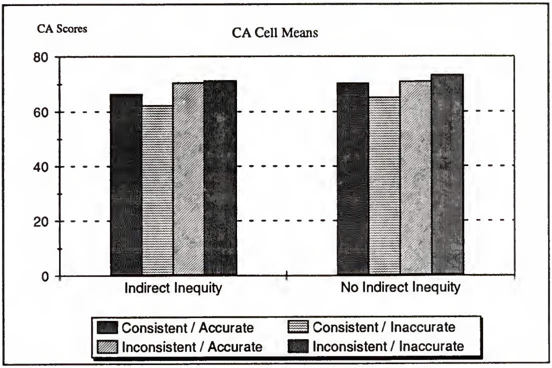

CA Scores
CA Cell Means
80
60
40
20
0
Indirect Inequity
No Indirect Inequity
Consistent / Accurate
Consistent / Inaccurate
Inconsistent / Accurate
Inconsistent / Inaccurate

Figure 5-15

CHAPTER 6
SUMMARY AND CONCLUSIONS

Introduction

This final chapter contains a summary of the hypotheses tested in this study and the results of the statistical tests conducted to test these hypotheses. In addition, the contributions, policy implications, and limitations of this study are addressed. Finally, this chapter concludes with a discussion of possible extensions to this research.

Summary and Discussion of Results

In this study, hypotheses concerning the effects of procedural injustice on taxpayers' attitudes and behavior were proposed and tested. Grounded in procedural justice theory, the hypotheses proposed in this study were related to two specific theoretical dimensions of procedural justice: consistency and accuracy. For each of these criteria of procedural justice, two hypotheses were advanced. The first predicted that a direct violation of the one of the criteria would cause taxpayers to be more acceptant of noncompliance with the tax laws. The second predicted that a direct violation of one of the standards would also cause a change in their behavior. Specifically, the second hypothesis predicted that taxpayers would reduce their level of tax compliance in response to a breach of the procedural justice criteria.

In addition to the hypotheses related to the direct effects of procedural injustice, two other hypotheses were advanced. They predicted that violations of procedural justice standards experienced indirectly by having knowledge of another's unfair treatment would produce the same effect on taxpayers' attitudes and behavior as direct violations of the procedural justice standards.

The results did not support the hypotheses concerning the effect of inconsistency on attitudes and behavior. Instead, the opposite effect was observed. Subjects who were exposed to inconsistent audit rates reported more small business income and were less acceptant of noncompliance than subjects who did not receive the manipulation. Moreover, these results were statistically significant without regard to how taxpaying behavior was measured or to the method of analysis employed.

There are several possible explanations for these unexpected results. Apparently, subjects whose audit rates were juxtaposed with lower audit rates framed the situation in such a way as to have higher subjective audit rates producing a deterrence effect. This effect overwhelmed any procedural justice effects that may have been produced by the experimental manipulation of audit consistency. Therefore, this confounding of procedural justice effects with the deterrent effect produced by contrasting audit rates makes it impossible to determine in this study how inconsistency affects taxpaying behaviors and attitudes.

Even if the manipulation of consistency in this study had not produced deterrence effects, it is not clear that the consistency manipulation in this study would have produced procedural justice effects strong enough to have produced the hypothesized effect on attitudes and behavior. There was not a strong consensus among subjects exposed to inconsistent audit rates that the rate differential in the experiment was unfair. Perhaps their ambivalence was due to the lack of an audit after the practice trial. Since there was no audit, subjects who believed they were subject to higher audit rates never suffered a financial penalty because of the apparent audit rate difference. Another possibility is that subjects may not have felt that inconsistent audit rates among members of the same community group in the experimental sessions were unfair since each group as a whole would not be penalized by the inconsistent audit rates. Finally, subjects may not have felt that inconsistent audit rates were inherently unfair without regard to these other explanations for their feelings.

Like the results for the consistency variable, the results for the accuracy variable fail to support the two hypotheses concerning the effect of tax enforcement procedures based upon inaccurate information. The results show that subjects who received a letter demanding a tax payment when none was due did not report significantly less business income in total and did not understate their business income more frequently than the group that didn't receive the letter. Also, the inaccuracy manipulation did not significantly affect attitudes towards compliance although the effect of the manipulation was in the hypothesized direction.

The results obtained for the indirect inequity variable, unlike those for the accuracy variable, support the prediction that indirect inequity reduces the level of tax compliance and agree with a prior survey study which obtained similar results (Kinsey 1992). Subjects who were informed that other participants had been treated unfairly by the taxing authority in the experiment reported less business income and understated business income more frequently than those who were not so informed. This effect was statistically significant when discrete measures of taxpaying behavior were used but not significant when continuous measures where used. This result was apparently caused by subjects who responded to the manipulation by frequently understating their business income but by small amounts.

However, when MANCOVA was used to analyze the effect of indirect inequity on taxpaying behavior, indirect inequity was no longer significant. Accordingly, the results obtained for the indirect inequity variable are sensitive to the statistical methodology used to assess its significance. Finally, as was the case with the accuracy variable, indirect inequity did cause subjects to be more acceptant of noncompliance, yet this effect was not statistically significant.

For the indirect inequity manipulation, taxpaying behavior was significantly affected, yet attitudes towards compliance were not. This disparity between attitudes and action may be due to the different incentives subjects face when giving an opinion on the propriety of hypothetical actions and when making a decision with financial consequences. The subjects, because of self-presentation concerns, may have answered that they did not agree with the propriety of understating income or overstating deductions when responding to questions about hypothetical scenarios, but when faced with the real economic rewards that came from understating income, their actions may have been incongruous with their statements of belief. Such behavior has been documented in prior research (Elffers et al. 1987).

While not the primary focus of this study, the results obtained for one of the two covariates included in the analysis of covariance, reactance and prior attitudes towards the IRS, indicate that individual differences may affect the response of taxpayers to tax enforcement efforts. Specifically, individual reactance as measured by the psychological reactance scale included in the experiment was significantly and positively related to subject's decisions to understate business income and to the number of periods that subjects understated business income. That is, subjects with high reactance scores tended to understate business income more frequently than those with lower scores.

Contributions and Implications

Research Contributions

The contributions of this study were made first by fully utilizing procedural justice theory to develop the hypotheses which were tested and second by employing a methodology that had not previously been used to test for procedural justice effects in a tax context. Prior research in this area has been limited by using ad hoc measures of procedural fairness and by relying upon survey methods alone in conducting the research.

The few studies which examine how procedural injustice affects the behavior of taxpayers allude to procedural justice theory, yet they do not take into account that procedural justice is a multi-dimensional concept. Consequently, the measures they use, such as whether IRS contact caused a taxpayer to pay more tax than was owed, capture broad perceptions of procedural justice but overlook specific dimensions of procedural fairness such as consistency and accuracy. By testing for the behavioral and attitudinal effects of consistency and accuracy, two specific criteria of procedural justice drawn from procedural justice theory, this study has been able to compare and contrast the separate effects of these two dimensions of procedural justice. As the results from this study show, taxpayers may respond differently according to which criteria of procedural justice is violated.

Much of the prior research concerning procedural justice effects on taxpayers has been conducted using surveys. This approach suffers from two well known limitations. First, using survey data, it is impossible to unequivocally show that procedural injustice causes taxpayers to evade taxes because respondents may use procedural injustice as an excuse for behavior they would have engaged in anyway. Second, because of self-portrayal concerns or due to the sensitive nature of personal tax questions, taxpayers may not admit to past tax cheating or to their intent to do so in the future when asked questions about their taxpaying behavior. Consequently, taxpaying behavior may not be accurately measured using surveys. By using an experimental lab setting to manipulate the variables of interest and to control any extraneous variables, this study establishes that there is a cause and effect relationship between some criteria of procedural injustice and taxpaying behavior. Also, by putting subjects into a setting where they made taxpaying decisions and responded to real economic incentives, a different and possibly more accurate measure of taxpaying behavior was obtained. Therefore, the methodological differences in this study make it possible to determine if prior research results in this area are invariant to the methodology used to obtain them.

The two shortcomings of survey research described above are found not only in prior research concerning procedural justice effects on taxpaying behavior but also in other types of tax compliance research. There are, however, two additional problems that are unique to the survey research which examines the effect of procedural justice judgements on tax compliance. First, prior survey research has shown that individuals who have had indirect contact with the IRS have decreased their level of tax compliance (Sheffrin and Triest 1992). Unfortunately, this result is subject to two possible interpretations: either contact with the IRS teaches taxpayers how to avoid getting caught or it leaves them feeling that the actions of the IRS were unfair. Second, the conclusions from prior survey research are applicable mainly to procedural injustice which is experienced indirectly rather than directly (Kinsey 1992, Smith 1992). This is the case because very few respondents to these surveys experienced procedural injustice personally. Furthermore, of those who had a personal experience, most did not earn the type of income that could be easily hidden from the IRS.

Using an experimental approach helped to circumvent these problems. Because experiential effects could be controlled during the experiment, this study was able to conclude that the reductions in the level of tax compliance observed in the results were caused by procedural justice concerns rather than experiential effects. Moreover, this study was able to examine the effect of direct procedural injustice on taxpaying behavior because the experimental setting made it possible to generate an adequate number of subjects who had personally been treated unjustly by a tax authority and who also had the opportunity to understate their income.

Policy Implications

Although preliminary, the results from this study have several implications for tax enforcement policy. In recent years, as a result of the Taxpayer Bill of Rights and its Compliance 2000 program, the IRS has attempted to improve the fairness of its enforcement policies. The results of this study indicate that as some of these institutional improvements are made, tax compliance may increase. This increase may not necessarily occur among those who experience direct contact with the IRS, however. Rather, the results obtained for indirect inequity suggest that such improvements may increase compliance among those who hear about IRS enforcement actions from others because they would be less likely to hear negative information about IRS actions. The effect of indirect inequity is important because it suggests that treating even a small number of taxpayers unfairly will have an negative impact on the compliance level of a much larger number of taxpayers as accounts of such incidents are related within taxpayers' social groups.

Limitations

Although the use of a simplified experimental setting is one of the contributions of this study, it also results in several limitations. The tax reporting decision which subjects made in the experiment was unambiguous. Either they would evade paying taxes or they would not. In actual practice, the decision to take a questionable deduction or not to report an income item may be supported by legal evidence. Such behavior is more like tax avoidance than tax evasion. Since this experiment does not capture tax avoidance behavior, the conclusions from this study cannot be generalized to behavior other than tax evasion. Also, because this study was conducted using an experimental setting, it is possible that the taxpaying choices subjects made in the experiment would not be made in a real tax reporting situation. For example, subjects in this study were responding to an incentive to raise money for community groups. To the extent this type of incentive differs from the personal financial incentives which taxpayers consider in making taxpaying decisions, the decisions subjects made in this study could differ from those they would make in a real taxpaying setting.

The specific manipulations of procedural injustice included in this study, audit rate inconsistency and inaccurate IRS correspondence, were not selected based upon prior knowledge that taxpayers are aware of these procedures or that they consider them to be unfair since no such evidence currently exists. Accordingly, to the extent taxpayers are unaware that IRS audit rates are inconsistent or that the IRS notices are frequently incorrect, the results of this study may not generalize to the behavior of actual taxpayers.

Future Research

This study examined the effects of two specific violations of procedural justice on taxpaying behavior: inconsistent audit rates and inaccurate IRS notices. Future research in this area could study how other forms of inconsistent or inaccurate tax enforcement procedures affect taxpayers' attitudes and behavior. In particular, a different manipulation of inconsistent tax enforcement policy, such as inconsistency in offering installment agreements or in allowing offers in compromise, could be used to study the affects of inconsistency independent of the deterrence effect produced in this study using inconsistent audit rates.

There are other procedural justice criteria which could affect taxpaying behavior. Perhaps, future research in this area could determine how violations of these criteria affect taxpaying behavior. For example, the correctability criterion, which essentially says there should be a fair appeal process, should also be important in a taxpaying setting given that taxpayers have an appeals process available to them.

Finally, the results obtained for the consistency manipulation in this study suggest that taxpayers' audit rate perceptions are heightened when they are contrasted with lower audit rates. This contrast effect has not been previously studied in the tax compliance literature. A future study in which the behavioral effects of contrasts of both higher and lower audit rates are compared to the effects of audits rates alone would contribute to the tax compliance literature.

APPENDIX A
EXPERIMENTAL MATERIALS

The materials contained in this appendix were given to subjects during the administration of the experiment with the exception of the letter of introduction and flyer which were sent to the head of each community organization who responded to a newspaper advertisement asking for community groups to participate in the experiment. The materials include a letter of introduction with its accompanying flyer, verbal instructions, written versions of the computer screens which subjects viewed, and the debriefing form which was given to subjects as they exited the computer lab.

INTRODUCTION LETTER AND ENCLOSURE

May 19, 1993

Mr. David Nicholson
1554 NW 21st Ave.
Gainesville, FL 32605

Dear Mr. Nicholson:

I want to thank you for your interest in having your group participate in my research study. As a doctoral candidate in the Business School at the University of Florida, I am currently conducting a study using interactive computer software to investigate how individuals make common household financial decisions. The study takes about one hour to complete and is held in Bryan Hall on the campus of the University of Florida. As part of the study, participants earn actual dollars. When community groups send volunteers, I donate their earnings to the sponsoring group. Community groups which send 10 people, 26 years of age or older, to the one hour decision making exercise earn approximately $300, those which send 20 earn approximately $650, and those which send 30 earn approximately $1050. In other words, if you can convince 30 people who are at least 26 years old to spend one hour participating in my study, your group would earn about $1050.

I have attached an information sheet which explains what I am doing in more detail. All of the sessions from now through June 5th are fully booked. I have reserved 20 spaces for your group on June 12. I will call you the first part of June to confirm your group's participation on that date. If you have any questions, call me at 336-4551.

Sincerely,

Ronald Worsham

Enclosure

Fund Raising Idea

Ron Worsham, a doctoral candidate in the College of Business at the University of Florida, has received a grant to study how people make personal financial decisions. To conduct the study, he needs both men and women volunteers 26 years of age or older to participate in a short decision-making exercise to be held every Saturday during the month of May and June in room 225, the graduate computer lab, in Byran Hall on the campus of the University of Florida. A map showing the location of the lab has been attached. Each Saturday, he will conduct one session from 10:00 a.m. until 11:00 a.m. and a second session from 12:00 p.m. until 1:00 p.m. However, on Saturday May 15th only, both sessions will begin one-half hour earlier. During each session, he will be able to accommodate approximately 25 people. The exercise will last approximately one hour, and participants will be asked to make common household financial decisions. Participants will only be required to attend one hour long session.

Volunteers will be paid for their participation according to their performance during the exercise. They will earn 15 dollars, on average, upon completing the one hour exercise and Ron is willing to donate this money to community groups which provide him with volunteers. In addition, he is willing to pay your group a bonus for every ten participants you provide. For the first ten people form a community group that come on campus to participate, he will pay an additional $150. For the second ten, he will pay another $200, and for the next ten another $250. Therefore, if you can convince ten people to come to UF to participate, your group would receive approximately $300. If twenty people come, your group would receive approximately $650, and if thirty people come to UF, your group would be paid approximately $1050. Those who choose not to complete the exercise will not be compensated; however, this is unlikely given that most people find the exercise to be enjoyable and non-threatening.

If you would like to participate in this fund raising project, or if you have any questions, please call Ron Worsham at 336-4551.

VERBAL INSTRUCTIONS

This research project is being conducted by Mr. Ronald Worsham, a doctoral candidate at the University of Florida. He may be contacted at 267 Business Building on the campus of the University of Florida.

As part of this exercise, you will be asked to answer several questions concerning your financial profile as well as your personality. You do not have to answer any question you do not wish to answer. In addition to answering questions, you will be asked to manage a portfolio of investments and to make other financial decisions using the computer. The exercise should take about one hour to complete.

You may withdraw from this exercise at any time without prejudice. However, if you decide to withdraw prior to the conclusion of the exercise, the computer you will use will be unable to calculate compensation for you. This means your group will not earn as much as it otherwise would have earned. You may ask questions concerning the procedures in this exercise either during or after the exercise.

All of your responses will identified by number and by the color of the group (red or blue) to which you have been assigned rather than by name. The color of your group is indicated by the color of the sign which has been placed near your computer. Your responses will be strictly confidential.

As a result of the decisions you make during the exercise, you will acquire a reserve of "cash". At the conclusion of this exercise, your group will be compensated according to the amount of "cash" you have on hand. This compensation will be determined by multiplying your "cash" by a conversion factor of 1/10,000 to arrive at the real dollars you will be paid. The average payout will be about fifteen dollars with the minimum being approximately nine dollars and the maximum being approximately nineteen dollars.

Some of you may not be familiar with computers. This should not be a problem. To use the computer during the exercise you must know how to locate the letter keys, the number keys and the enter or return key. The number and letter keys are arranged on the keyboard as they are on a standard typewriter. The enter or return key is found at the right hand side of the keyboard. Please take a moment to locate these keys. If you are unable to find them, raise your hand.

As you arrived you each received a packet of materials in addition to these instructions. The items in the packet include a set of instructions you may use while operating the computer, a set of envelopes, and a sheet of paper labeled INVESTMENT INCOME. Please take a moment to locate these items. If you are missing any of these items, please raise your hand.

Now type the word the proctor gives you and then press the return key. At this point, the computer will load the exercise. You may then begin. Please do not communicate with any of the other participants after you have started the exercise. If you are unable to start the exercise or if you have problems with the computer during the exercise, please raise your hand.

COMPUTER SCREENS

The text below displays the screens which subjects viewed during the administration of the experiment. However, the explanatory notes in italics found herein were not viewed by the subjects.

SCREEN #1

INPUT SUBJECT

Please enter any six numbers you desire and hit the RETURN key when you are finished (you can use the backspace key for corrections). These six digits will serve as an I.D. number in coding your responses.

SCREEN #2

INPUT NAME AND ADDRESS

During this exercise, you will use a name and address other than your own. Use your imagination to create a new name and address for yourself. Please enter your new name and address in the spaces provided below. When you are finished with each line, hit the RETURN key (you can use the backspace key for corrections):

First Name: ....
Last Name: ....

Street Address: ....
City: ....
Zip Code: ....

The following appeared after entry of the zip code:

If you are satisfied with what you have entered and wish to continue press Y. If not, press N and you'll be allowed to make changes before continuing.

SCREEN #3

PURPOSE OF THIS STUDY

The purpose of this study is to determine how people make decisions. During this exercise you will have the opportunity to choose among different investment alternatives. Your task will be to manage the portfolio of investments you choose as successfully as possible.

Press RETURN to continue.

SCREEN #4

OVERVIEW

There are three parts to this exercise. The first part contains a brief questionnaire, instructions and a practice trial. In the second part, you will be asked to make investment and taxpaying decisions. In the third part, you will answer some general questions.

All your responses will be collected and stored on the program disk. Simply respond to the prompts which appear on screen.

Until you have left the room, please do not discuss this exercise with any of the other participants.

Press RETURN to continue.

SCREEN #5

PART 1 - QUESTIONNAIRE

Please answer the following questions. After answering each question, press the RETURN key to move to the next question (you may use the backspace key for corrections).

SCREEN #6

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I have a strong desire to maintain my personal freedom.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #7

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I enjoy playing "devil's advocate" whenever I can.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #8

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

If I receive a lukewarm dish at a restaurant, I make an attempt to let that be known.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #9

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

You probably would end up in jail for at least five years if you tried to bribe an IRS employee.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #10

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

I have the "know-how" to invest on my own without the help of a stockbroker or other professionals.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #11

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

In discussions, I am easily persuaded by others.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #12

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

Nothing excites me as much as a good argument.

Strongly Disagree Strongly Agree

1 2 3 4

SCREEN #13

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I resent authority figures who try to tell me what to do.

Strongly Disagree Strongly Agree

1 2 3 4

SCREEN #14

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

I am confident that the IRS would never try to take more money from me than they should.

Strongly Agree Strongly Disagree

1 2 3 4 5 6

SCREEN #15

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

I am willing to accept more than a moderate amount of risk when investing.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #16

For the statement below, please choose the appropriate number where "1" means "strongly disagree and "4" means "strongly agree" as to how you feel about the statement:

I find that I often question authority.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #17

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

It would be better to have more freedom to do what I want on a job.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #18

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

When it comes to investigating their own people, the IRS is as thorough as they are with everyone else.

Strongly Agree Strongly Disagree

1 2 3 4 5 6

SCREEN #19

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

I frequently invest in investments other than certificates of deposit or savings bonds.

Strongly Agree Strongly Disagree

1 2 3 4 5 6

SCREEN #20

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I enjoy seeing someone else do something that neither of us is supposed to do.

Strongly Disagree Strongly Agree

1 2 3 4

SCREEN #21

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

If I am told what to do, I often do the opposite.

Strongly Disagree Strongly Agree

1 2 3 4

SCREEN #22

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I am sometimes afraid to disagree with others.

Strongly Disagree Strongly Agree

1 2 3 4

SCREEN #23

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

It really bothers me when police officers tell people what to do.

Strongly Disagree Strongly Agree

1 2 3 4

SCREEN #24

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

It does not upset me to change my plans because someone in the group wants to do something else.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #25

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

The IRS employees are honest--you could never bribe them.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #26

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Age is a factor which should be considered in forming an overall investment strategy.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #27

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I don't mind other people telling me what to do.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #28

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I enjoy debates with other people.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #29

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

One should invest in the stocks or in mutual funds only when the interest rate on certificates of deposit is low.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #30

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

If some one asks a favor of me, I will think twice about what this person is really after.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #31

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I am not very tolerant of others' attempts to persuade me.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #32

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I often follow the suggestions of others.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #33

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

It is generally a good idea to spread your investment dollars among several different types of investments.

Strongly Agree / Strongly Disagree

1 2 3 4 5 6

SCREEN #34

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I am relatively opinionated.

Strongly Disagree / Strongly Agree

1 2 3 4

SCREEN #35

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

It is important to me to be in a powerful position relative to others.

Strongly Disagree / Strongly Agree

1 2 3 4

SCREEN #36

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I am very open to solutions to my problems from others.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #37

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I enjoy "showing up" people who think they are right.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #38

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

The stock of smaller companies tends to yield larger returns than the stock of large, well known companies.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #39

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I consider myself more competitive than cooperative.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #40

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I don't mind doing something for someone even when I don't know why I'm doing it.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #41

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I usually go along with others' advice.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #42

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I feel it is better to stand up for what I believe than to be silent.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #43

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

I am very stubborn and set in my ways.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #44

For the statement below, please choose the appropriate number where "1" means "strongly disagree" and "4" means "strongly agree" as to how you feel about the statement:

It is important for me to get along well with the people I work with.

Strongly Disagree			Strongly Agree
1	2	3	4

SCREEN #45

PART 1 - INSTRUCTIONS

This is a simulation produced by Worsham's Business Research to investigate personal financial decision making behavior. You are to imagine that you are the owner of small, cash-only business. Because your regular job provides enough income to meet your needs, your small business will be a source of extra cash which you may either keep on hand or invest in several investment opportunities. You will be asked to make a series of decisions over a multi-year period. For example, during each year you will have to decide how much, if any, of your extra cash from the prior year's small business and investment earnings you will invest and where among the available investment opportunities you will place your investment. At the end of each year, you will also be asked to file a tax return. In completing the return, you will be asked to decide how much of the income from your small business and from your investments you will report.

Press RETURN to continue.

SCREEN #46

Your purpose in this simulation is to manage the income from your business as successfully as possible. Your compensation for participating in this simulation will depend upon how well you do this. You have been given a written copy of these instructions which you may refer to at any time.

Press RETURN to continue.

SCREEN #47

INVESTMENTS

At the beginning of each year, you will be told how much cash you have available to invest. This cash will have come from your small business and investment income earned in prior years reduced by tax payments.

Each year, you will be given the option of allocating your cash among three investment options. Also, you will be given the option of retaining some or all of your cash. Your investment options will vary from one year to the next. The next screen lists the investment opportunities that will be available to you at one time or another during the exercise and their associated risk and return characteristics.

Press RETURN to continue.

SCREEN #48

INVESTMENT OPPORTUNITIES

CERTIFICATE OF DEPOSIT

Probability	Return
Certain	4%

MUTUAL FUND A

Probability	Return
3 in 4	10%
1 in 4	2%

MUTUAL FUND B

Probability	Return
2 in 4	16%
2 in 4	1%

MUTUAL FUND C

Probability	Return
4 in 5	11%
1 in 5	0%

MUTUAL FUND D

Probability	Return
3 in 5	12%
2 in 5	3%

Press RETURN to continue.

SCREEN #49

The certificate of deposit yields a certain return of 4% per year every year. However, the other investment options each have two possible returns. The likelihood of each return occurring was stated in terms of a probability. For example, you read that the mutual fund A gives a 10% return with a probability of 3 in 4. This means that, on average, mutual fund A will give a return of 10% three out of every four years. Also, mutual fund A gives a 2% return with a probability of 1 in 4. Therefore, mutual fund A, on average, yields a return of

2% one out of every four years. You have read that mutual fund B generates two possible returns each with a probability of 2 in 4. Again, this should be interpreted to mean that, on average, each return will occur two out of every four years. Mutual fund C will yield a return of 11%, on average, four out of every five years, and zero returns, on average, one out of every five years. Finally, mutual fund D will yield a return of 12%, on average, three out of every five years, and a 3% return, on average, 2 out of every five years. You may refer to the schedule of investment opportunities in your copy of the written instructions at any time during this experiment.

Press RETURN to continue.

SCREEN #50

TAXATION

At the end of each year, the total cash receipts from your small business and from your investments will be calculated. You will be asked to decide how much of each category of income you will report on your tax return. The tax rate is 28%.

Also at the end of each year, the companies sponsoring the investments you have selected will report the amount of investment income you have earned to the IRS. The IRS will compare the amount of investment income you report on your tax return to the amount the investment companies say you earned during the year. This means that the IRS will always know when you fail to report all of your investment income. If you don't report all of the investment income you have earned, the IRS will charge you with a penalty equal to 50% of the tax evaded and you will also be required to pay the amount of tax evaded.

Press RETURN to continue.

SCREEN #51

The IRS will take a different approach with the amount of small business income you report. Each year, audits will be conducted according to a random sample, and fines imposed on tax evaded in that year.

Press RETURN to continue.

SCREEN #53

Unless the computer indicates that you are being audited, you will be safe from the possibility of an audit until the next year. If you are selected to have your small business income for the current year audited, the computer will display this information. At this point, your computer will not know how much income you actually earned from your small business. This information was given to you in the envelopes at the beginning of this exercise. To allow the IRS to conduct audits on those who have been selected, everyone will place the slip of paper which displays their small business income for the year back into the envelope provided and deliver the unsealed envelope to the proctor. The proctor will input this information for those being audited at a central computer which will transmit this information electronically to the computer you are using. Then, only if you have been selected for an audit, your computer will compare the amount of small business income you earned for the year with the amount you reported. At the same time, the proctor will also input the small business income for everyone who was not selected for an audit so that this information can be transmitted to their computers to update their cash balances.

Press RETURN to continue

SCREEN #54

If you are audited AND you underreported small business income then, for that year only, you will be charged a penalty equal to 50% of the amount of the tax evaded. In addition to the fine, you will also be required to pay the amount of tax evaded. Also, you will be assessed an audit cost which represents the cost of hiring professionals to assist you in responding to the audit and your own lost time and frustration. On average, this cost will be $1000, but may be higher or lower than this in your own particular situation. If you are audited BUT you reported all of your small business income, you will not have to pay additional taxes or penalties, but you will still have to pay the audit cost described above to pay for professional help.

Press RETURN to continue.

SCREEN #55

COMPENSATION

At the conclusion of this experiment you will be compensated for your participation. As discussed in the oral instructions you were given, the compensation you receive will depend upon the amount of cash you have remaining at the end of this exercise. The actual cash you receive will be a predetermined percentage of the cash you accumulate during this exercise.

At the end of each year, you will be shown how much cash you have earned for purposes of this exercise and how it translates into real dollars you will receive.

Press RETURN to continue.

SCREEN #56

To help you understand how dollars earned in the exercise will be converted to real dollars, the table on the next screen summarizes the results of the group which participated in this exercise before you.

The first column of the table contains the "cash" which the participants had accumulated by the end of the exercise. This experimental cash was converted to real dollars by multiplying the experimental dollars by 1/10,000 which is the conversion factor given to you in the oral instructions. The resulting amount of real dollars earned by each participant is found in the second column. Finally, the amount of real dollars that each participant actually received is listed in the last column.

Press RETURN to continue

SCREEN #57

Payout Table for Group One

	Experimental Dollars	X Conversion Factor	= Real Dollars	Amount Paid
Subject 1	105190.75	1/10000	10.52	10.52
Subject 2*	29345.00	1/10000	*2.93	*2.93
Subject 3	128067.78	1/10000	12.81	12.81
Subject 4	93356.27	1/10000	9.34	9.34
Subject 5*	21091.33	1/10000	*2.11	*2.11
Subject 6*	11765.00	1/10000	*1.18	*1.18
Subject 7	175016.00	1/10000	17.50	17.50
Subject 8	162451.00	1/10000	16.25	16.25
Subject 9*	35874.00	1/10000	*3.59	*3.59
Subject 10	107543.00	1/10000	10.75	10.75
Subject 11*	18742.00	1/10000	*1.87	*1.87
Subject 12	102456.00	1/10000	10.25	10.25
Subject 13*	13172.00	1/10000	*1.32	*1.32
Subject 14*	19876.87	1/10000	*1.99	*1.99

*Our IRS misrecorded their tax payments and liabilities. Because of this problem, the IRS charged these people with penalties for not reporting their small business income even though they had reported everything.

This table was viewed by those subjects who received the indirect inequity treatment.

SCREEN #58

Payout Table for Group One

	Experimental Dollars	X Conversion Factor	= Real Dollars	Amount Paid
Subject 1	105190.75	1/10000	10.52	10.52
Subject 2	129345.00	1/10000	12.93	12.93
Subject 3	128067.78	1/10000	12.81	12.81
Subject 4	93356.27	1/10000	9.34	9.34
Subject 5	121091.33	1/10000	12.11	12.11
Subject 6	111765.00	1/10000	11.18	11.18
Subject 7	175016.00	1/10000	17.50	17.50
Subject 8	162451.00	1/10000	16.25	16.25
Subject 9	95874.00	1/10000	9.59	9.59
Subject 10	107543.00	1/10000	10.75	10.75
Subject 11	118742.00	1/10000	11.87	11.87
Subject 12	102456.00	1/10000	10.25	10.25
Subject 13	113172.00	1/10000	11.32	11.32
Subject 14	119876.87	1/10000	11.99	11.99

Those who did not receive the Indirect Inequity treatment saw this screen rather than the one before it.

SCREEN #59

You have been given a written copy of these instructions. Feel free to refer to them during the simulation. Before the simulation begins, you will go through a practice trial which will help you become familiar with tasks you will perform during the actual simulation. Your responses during the practice trial will not be recorded and will not be considered in determining your compensation. If you have any questions during the practice trial, raise your hand to obtain help from the proctor. When you are ready to begin the practice trial, press RETURN.

SCREEN #60

PART 1 - PRACTICE TRIAL

The practice trial has begun. You have $10,000 to invest.

Press RETURN to continue.

SCREEN #61

Options	Amount
A. Keep some of the Cash	0
A. Certificate of Deposit	0
B. Mutual Fund A	0
C. Mutual Fund B	0
Total Allocated so Far	0
Total to be Allocated	10,000

Please allocate the cash you have among the available options by typing the letter corresponding to the option you desire and then typing the amount. To go to the next option, press the letter corresponding to that option and so on. When you are satisfied with the allocation, hit RETURN to continue.

During the practice trial and for the periods that followed, the first three investment options consisted of some combination of the five investments on screen #48. Each year the combination of investments differed so as to avoid having the investment decision become routine.

SCREEN #62

The table below lists your income from investments during the practice trial:

PRACTICE TRIAL INVESTMENT INCOME

Investment	Return	Income
Cash Kept on Hand	0%	0.00
Certificate of Deposit	4%	10.25
Mutual Fund A	2%	200.00
Mutual Fund B	16%	200.00
Total Investment Income		410.25

PLEASE MAKE A COPY OF THIS SUMMARY ON THE PAPER PROVIDED

Press RETURN to continue.

The percentage returns above were the same for all participants during the practice trial.

SCREEN #63

Did you record your investment income on the paper provided? If you did great! If not, please do so before proceeding.

Press RETURN to continue.

SCREEN #64

Please take the slip of paper out of the envelope labeled SMALL BUSINESS INCOME-PRACTICE to find out how much small business income you earned during the practice trial.

Press RETURN to continue.

SCREEN #65

You may now file your tax return for the practice trial using the investment income you just finished copying on paper and the small business income on the slip of paper you pulled from the envelope. To do so, enter the amount of income you wish to report on the appropriate line of the form labeled TAX RETURN. After you have typed in each amount, press the RETURN key. You will be allowed to make changes if you want to.

TAX RETURN

Line 1: Investment Income	0
Line 2: Business Income	0
Total Income Reported	0
Taxes Due (28% Rate)	0

After Line 2 was completed, the following sentence appeared if all investment income was reported; otherwise the paragraph bellow it appeared.

You have now completed your tax return for the practice trial. Press F to file the return as it is, or press R to review the return and make any changes.

You have now completed your tax return for year #1. However, you have not reported all of your investment income for year #1. Remember, the IRS will compare your actual investment income to the amount you report every year. Press F to file the return as it is, or press R to review the return and make any changes.

SCREEN #66

Your tax return has been filed and been processed.

The amount of small business income you reported for the practice trial will be audited.

Press RETURN to continue.

SCREEN #67

Return the slip of paper containing your small business income for this year to the envelope labeled SMALL BUSINESS INCOME-PRACTICE and deliver the envelope to the proctor. After he or she has received this information, he or she will give you a password which you will need to type in below to continue. Please do not reveal the password to anyone.

Please type in password:

SCREEN #68

The audit of the small business income you reported for the practice trial has been completed. Since you reported all your small business income, you were not required to pay any additional taxes or penalties. However, you spent 1000 dollars for accountant's fees and for the other costs you incurred to respond to the IRS audit.

Press RETURN to continue.

In the practice trial, those who reported all of their small business income were shown screen #68. Otherwise, they viewed screen #69.

SCREEN #69

The audit of the small business income you reported for the practice trial has been completed. Because you failed to report all of your small business income for the practice trial, you must pay additional tax and a failure to report penalty. Also, you were required to make cash payments to your accountant and for other costs you incurred to respond to the IRS audit. The cash payments you made as a result of the audit are computed below:

Your Business Income	15,000.00
Less: Business Income You Reported	9589.75
Your Unreported Business Income	6410.25
Tax on Unreported Business Income (28%)	1794.87
Penalty (Half the Unpaid Tax)	897.43
Audit Costs	1000.00
Total Cash Paid	3692.30

SCREEN #70

The following is a summary of your dealings with the IRS during the practice trial:

TAX SUMMARY FOR PRACTICE TRIAL

Investment Income Reported	410.25
Business Cash Income Reported	9,589.75
Total Income Reported	10,000.00
Taxes on Reported Income	2,800.00
Taxes on Unreported Income	1794.87
Penalties	897.43
Audit Costs	1,000.00
Tax-Related Cash Payments	6492.30

Press RETURN to continue.

SCREEN #71

For this practice trial, the small business income you reported was audited to help you understand the consequences of being selected for an audit of small business income. However, once the exercise begins, audits of small business income will be conducted randomly.

Press RETURN to continue.

Subjects who received the inconsistency manipulation viewed screens #72 and #73 below. Those who were not assigned to the inconsistency treatment viewed screen #74 below.

SCREEN #72

Each year, audits of small business income will be conducted according to a random sample; however, your audit rate will depend on the color of your group. Your group, the blue/red group will face an audit rate of one in ten each year. This means that, on average, one out of every ten returns filed by members of the blue/red group will be audited each year. The other group, the red/blue group, will face an audit rate of only one in thirty each year. This means that, on average, only one out of every thirty returns filed by members of the red/blue group will be audited each year. Therefore, the odds small business income being audited are three times greater for you than for a member of the red/blue group. Other than this difference in audit rates, members of both the red and the blue groups will see and do the same things as they make investing decisions and file tax returns.

Press RETURN to continue.

SCREEN #73

The chart below summarizes the audit rates on the small business income of the two groups participating in this exercise. Remember, you are in the blue/red group. Therefore your audit rate on small business income is three time higher than the same rate for members of the red/blue group.

Group	Audit Rate
Red	1 out of 30
Blue	1 out of 10

Press RETURN to continue.

SCREEN #74

Each year audits of small business income will be conducted according to a random sample. You will face an audit rate of one in ten each year on the small business income you report. This means that, on average, only one out of every ten returns filed by participants in this exercise will be audited each year.

Press RETURN to continue.

SCREEN #75

The following table is a summary of all your activities during the practice trial:

Cash on Hand at Beginning of Practice Trial		10,000.00
Plus Cash Receipts:		
Small Business Income	15,000.00	
Cash Kept on Hand	0.00	
Certificate of Deposit	10.25	
Mutual Fund A	200.00	
Mutual Fund B	200.00	15,410.25
*Tax-Related Payments	6,492.30	(6,492.30)
Cash at End of Practice Trial		18,917.95

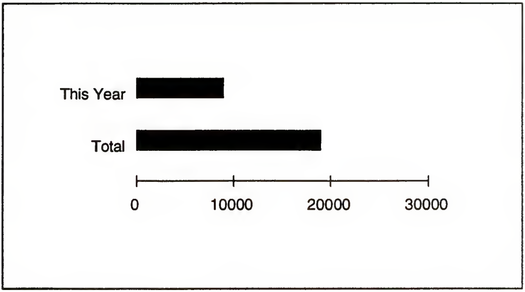
*This consists of taxes and applicable penalties and audit costs.

Press RETURN to continue.

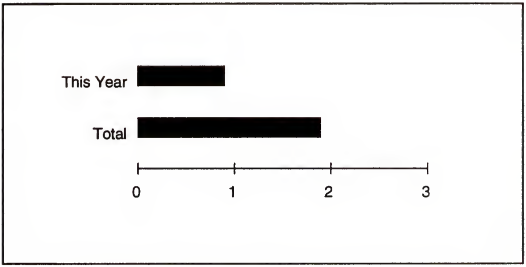
SCREEN#76

The two graphs below show you how much experimental cash and real cash you have accumulated during this practice session. Note, the bars labeled "Total" display your current earnings plus the amount of cash you had at the beginning of the practice trial. The cash you have at the end of this practice trial will not be included in your earnings once the exercise begins.

Experimental Cash



Real Cash



SCREEN#77

This concludes the practice trial. The next screen which you see will begin the actual business simulation.

Press RETURN to continue.

SCREEN #78

PART 2 - INVESTMENT EXERCISE

The first year has begun. You have $33,000 to invest.

Press RETURN to continue.

Participants who were assigned to the inaccuracy treatment received an additional $955 which is approximately the present value of the additional payment they made as a result of receiving a letter in the third period demanding the payment of additional taxes and penalties.

SCREEN #79

Options	Amount
A. Keep some of the Cash	0
B. Certificate of Deposit	0
C. Mutual Fund A	0
D. Mutual Fund B	0
Total Allocated so Far	0
Total to be Allocated	33,000

Please allocate the cash you have among the available options by typing the letter to the left of the option you desire and then typing the amount. To go to the next option, press the letter to the left of that option and so on. When you are satisfied with the allocation, hit RETURN to continue.

SCREEN #80

The first year is now over. The table below lists your income from investments during the year. These amounts have been reported by the investment companies to the IRS.

FIRST YEAR INVESTMENT INCOME

Investment	Return	Income
Cash Kept on Hand	0%	0.00
Certificate of Deposit	4%	10.25
Mutual Fund A	2%	200.00
Mutual Fund B	16%	200.00
Total Investment Income		410.25

PLEASE MAKE A COPY OF THIS SUMMARY ON THE PAPER PROVIDED

Press RETURN to continue.

The returns subjects viewed on this screen reflected probablistic outcomes. Also, the words designating the period changed from period to period.

SCREEN #81

Did you record your investment income on the paper provided? If you did great! If not, please do so before proceeding.

Press RETURN to continue.

SCREEN #82

Please take the slip of paper out of the envelope labeled SMALL BUSINESS INCOME-YEAR ONE to find out how much small business income your earned in year one.

Press RETURN to continue

SCREEN #83

You may now file your tax return for year one using the investment income you just finished copying on paper and the small business income on the slip of paper you pulled from the envelope. To do so, enter the amount of income you wish to report on the appropriate line of the form labeled TAX RETURN. After you have typed in each amount, press the RETURN key. You will be allowed to make changes if you want to.

TAX RETURN

line 1: Investment Income	0
Line 2: Business Income	0
Total Income Reported	0
Taxes Due (28% Rate)	0

After Line 2 has been completed, the following message appeared:

You have now completed your tax return for year #1. Press F to file the return as it is, or press R to review the return and make any changes.

If the participant has not reported all of the investment income earned during the year, the following message appeared in lieu of the message above:

You have now completed your tax return for year #1. However, you have not reported all of your investment income for year #1. Remember, the IRS will compare your actual investment income to the amount you report every year. Press F to file the return as it is, or press R to review the return and make any changes.

SCREEN #84

Your tax return has been filed and been processed.

The amount of small business income you reported for year #1 will not be audited.

Press RETURN to continue.

Screen #84 appeared in all periods except for the practice trial since no audits of small business income were conducted after the practice trial.

SCREEN #85

Return the slip of paper containing your small business income for this year to the envelope labeled SMALL BUSINESS INCOME-YEAR #1 and deliver the envelope to the proctor. After he or she has received this information, he or she will give you a password which you will need to type in below to continue. Please do not reveal the password to anyone.

Please type in password:

The next five screens which follow only appeared at the end of period three for those subjects who received the inaccuracy manipulation.

SCREEN #86

Also, after filing your tax return for year three, you recently received the notice from the IRS on the next screen.

Press RETURN to continue

SCREEN #87

Date

Participant's Name
Participant's Address
Participant's Subject Number

Dear Taxpayer:

The amount of investment income reported on your recently filed tax return does not agree with our records. You have reported X but our records indicate your first year investment income was X + 2000. Accordingly, we are assessing you additional taxes in the amount of $560 and penalties of $280. Please remit a total check to us for $840 within ten days of the date of this notice. Your failure to respond to this notice will constitute grounds for further enforcement action.

Sincerely,

IRS Field Office
Jacksonville, Florida

Press RETURN to continue

Each letter was personalized with the name, address and subject number entered by subjects at the beginning of the exercise.

SCREEN #88

You may respond to this notice in one of two ways. First, you may pay the additional taxes and penalties the IRS is requesting. Second, you may contest this assessment. If you contest, you may or may not be able to resolve this problem. Also, it will cost you $300, on average, to hire a CPA to contest this assessment.

If you would like to contest the IRS notice press "C". If you would rather pay $840 which is what the IRS is asking for press "P".

If participants contested, they viewed screen #89; otherwise, they viewed screen #90.

SCREEN #89

You hired a CPA to write a letter to the IRS to contest. The IRS has received the letter, but they insist the additional $840 in taxes and penalties are correct. Further, they have since levied your bank account to obtain the amount they demanded. Also, the CPA has billed you $300 for her services. These amounts will be reflected in your tax summary on the next screen.

Press RETURN to continue.

SCREEN #90

Your payment has been mailed to the IRS.

Press RETURN to continue.

SCREEN #91

The following is a summary of your dealings with the IRS in year #1:

TAX SUMMARY FOR YEAR ONE

Investment Income Reported	410.25
Business Cash Income Reported	9,589.75
Total Income Reported	10,000.00
Taxes on Reported Income	2,800.00
Taxes on Unreported Income	0
Penalties	0
Audit Costs	0
Tax-Related Cash Payments	2,800.00

Press RETURN to continue.

The penalties and audit costs above were displayed for those subjects not receiving the inaccuracy treatment. Those who received the inaccuracy treatment were shown penalties and audit costs reflecting the amounts in screen #89.

SCREEN #92

The following table is a summary of all your activities during year #1:

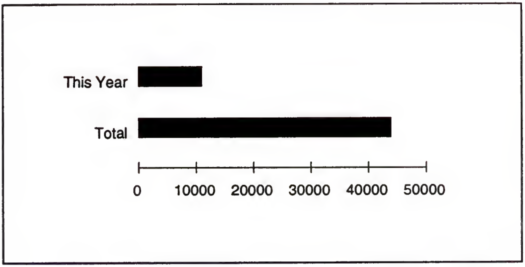
Cash on Hand at Beginning of Year One		33,000.00
Plus Cash Receipts:		
Small Business Income	13,300.00	
Cash Kept on Hand	0.00	
Certificate of Deposit	10.25	
Mutual Fund A	200.00	
Mutual Fund B	200.00	13,710.25
Less Cash Payments:		
Tax-Related Payments	2,800.00	(2,800.00)
Cash Available to Invest Next Year		43,910.25

Press RETURN to continue.

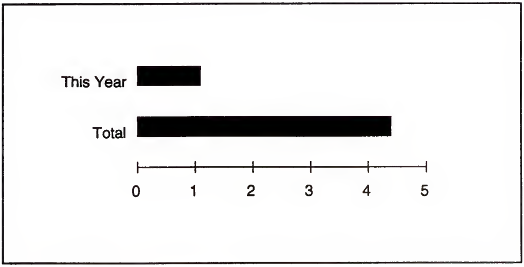
SCREEN #93

The two graphs below show you how much experimental cash and real cash you have accumulated during year #1. Note, the bars labeled "Total" display your current earnings plus the amount of cash you had at the beginning of year #1.

Experimental Cash



Real Cash



Press RETURN to continue.

SCREEN #94

Year #2 has begun. You have $43,910.25 to invest which was carried over from the prior year.

Press RETURN to continue.

From this point forward the screens were the same every period except the year name and number changed from period to period. After the seventh period, the following sequence of screens appeared:

SCREEN #95

PART 3 - GENERAL QUESTIONS

Please answer the following questions. After answering each question, press the RETURN key to move to the next question (you may use the backspace key for corrections).

SCREEN #96

For the statement below, please choose the appropriate number where "1" means "perfectly acceptable" and "6" means "not at all acceptable" as to how you feel about the statement:

Trading or exchanging goods or services with a friend or neighbor and not reporting it on your tax form.

Perfectly Acceptable					Not at all Acceptable
1	2	3	4	5	6

SCREEN #97

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

By accepting more risk, an investor will generally earn higher returns.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #98

For the statement below, please choose the appropriate number where "1" means "perfectly acceptable" and "6" means "not at all acceptable" as to how you feel about the statement:

Reporting your main income fully, but not including some small outside income.

Perfectly Acceptable					Not at all Acceptable
1	2	3	4	5	6

SCREEN #99

For the statement below, please choose the appropriate number where "1" means "perfectly acceptable" and "6" means "not at all acceptable" as to how you feel about the statement:

Being paid in cash for a job and then not reporting it on your tax form.

Perfectly Acceptable					Not at all Acceptable
1	2	3	4	5	6

SCREEN #100

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Only those who can afford to lose money should invest in anything other than a certificate of deposit or a savings account.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #101

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Stock mutual funds seem to be a better hedge against inflation than other investments like certificates of deposit which earn interest.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #102

For the statement below, please choose the appropriate number where "1" means "perfectly acceptable" and "6" means "not at all acceptable" as to how you feel about the statement:

Not reporting some earnings from investments or interest that the government would not be able to find out about.

Perfectly Acceptable					Not at all Acceptable
1	2	3	4	5	6

SCREEN #103

For the statement below, please choose the appropriate number where "1" means "perfectly acceptable" and "6" means "not at all acceptable" as to how you feel about the statement:

Adding a little bit more than you actually spend when reporting your business expenses.

Perfectly Acceptable					Not at all Acceptable
1	2	3	4	5	6

SCREEN #104

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

You really don't have to know very much to be a successful investor.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #105

For the statement below, please choose the appropriate number where "1" means "perfectly acceptable" and "6" means "not at all acceptable" as to how you feel about the statement:

Stretching medical deductions to include some expenses which are not really medical.

Perfectly Acceptable					Not at all Acceptable
1	2	3	4	5	6

SCREEN #106

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Most of my investment ideas come from other people.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #107

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Since a lot of rich people pay no taxes at all, if someone like me underpays a little, it's not a big deal.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #108

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Tax rates are just too high, so it's not really cheating when you find ways to pay less than you're supposed to.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #109

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

I would be willing to invest based upon the recommendation of a financial publication like the Wall Street Journal without looking at other sources of information.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #110

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

When you're not really sure whether or not you deserve a tax deduction, it makes sense to take a chance and take the deduction anyway.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #111

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

One should only make risky investments after accumulating a six month cash reserve.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #112

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

With what things cost these days, it's okay to cut a few corners on your tax form just to help make ends meet.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #113

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

It's not so wrong to hold back a little bit on taxes since the government spends too much anyway.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #114

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

In the exercise, the mutual funds seemed to be riskier than the certificate of deposit.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #115

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

When you know you deserve a deduction they won't let you take, it makes sense to take it some other place where they won't catch you on it.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #116

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

It is not so wrong to underreport certain income since it does not really hurt anyone.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #117

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

The chances of getting caught are so low that it is worthwhile trying to cut corners a little on your taxes.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #118

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

Small investors ought to stay clear of the stock market because the risks are too high.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #119

For the statement below, please choose the appropriate number where "1" means "strongly agree" and "6" means "strongly disagree" as to how you feel about the statement:

It's all right to occasionally underreport certain income or claim an undeserved deduction if you are generally a loyal and law-abiding citizen.

Strongly Agree					Strongly Disagree
1	2	3	4	5	6

SCREEN #120

Which of the following words best describes your feelings toward this exercise?

A. Very uninteresting

B. Uninteresting

C. Interesting

D. Very Interesting

Please type the appropriate letter to answer:

SCREEN #121

Please choose the appropriate number where "1" means "very unrealistic" and "5" means "very realistic" as to how realistic you felt the simulation was:

Very Unrealistic				Very Realistic
1	2	3	4	5

SCREEN #122

Please briefly describe what you think the experiment was about. You have up to 10 lines for your answer. Backspace for corrections. The cursor will automatically move from the right side of the screen to the left side of the screen when you reach the right margin. Hit RETURN twice to quit.

SCREEN #123

Would you make the same taxpaying decisions in the real world that you made in this simulation?

Press Y to answer Yes or press N to answer No:

SCREEN #124

Please briefly describe how you would behave differently. You have up to 10 lines for your answer. Backspace for corrections. The cursor will automatically move from the right side of the screen to the left side of the screen when you reach the right margin. Hit RETURN twice to quit.

SCREEN #125

According to the instructions, was your audit rate on small business income higher than, lower than or the same as the audit rate on the small business income of the other group participating in this exercise?

A. Higher than

B. Lower than

C. The same as

Please type the appropriate letter to answer:

SCREEN #126

Please choose the appropriate number where "1" means "very unfair" and "5" means "very fair" as to how you would describe the difference between your audit rate on small business income and the same audit rate for the other group in this exercise:

Very Unfair				Very Fair
1	2	3	4	5

Screens #125 and #126 were presented only to those subjects who received the inconsistency manipulation.

SCREEN #127

Was the letter you received at the end of the third year correct or incorrect concerning your failure to report your investment income?

A. Correct

B. Incorrect

Please type the appropriate letter to answer:

SCREEN #128

Please choose the appropriate number where "1" means "very unfair" and "5" means "very fair" as to how you would describe having to pay additional taxes and penalties or audit costs as a result of receiving the letter:

Very Unfair				Very Fair
1	2	3	4	5

Screens #127 and #128 were presented only to those subjects who received the inaccuracy manipulation.

SCREEN #129

Did all of the participants who have previously participated in this exercise earn as much as they should have?

Please type "N" to indicate no and "Y" to indicate yes:

SCREEN #130

Please choose the appropriate number where "1" means "very unfair" and "2" means "very fair" as to how you would describe how fairly the prior participants were compensated:

Very Unfair				Very Fair
1	2	3	4	5

Screens #129 and #130 were presented only to those subjects who received the indirect inequity manipulation.

SCREEN #131

What is your estimate of the number of years you have filed your own tax return?

Please type the number of years:

SCREEN #132

What is your gender?

Enter "M" if you are male or "F" if you are female:

SCREEN #133

Please enter your age:

SCREEN #134

What is your estimate of your annual income (if married, your joint annual income)?

A. Somewhere between zero and fifteen thousand dollars

B. Somewhere between fifteen thousand and thirty thousand dollars

C. Somewhere between thirty thousand and sixty thousand dollars

D. Over sixty thousand dollars

Please type the appropriate letter to answer::

SCREEN #135

If your annual income were to increase this year by one dollar, what is your estimate of the tax rate that would apply to that additional dollar?

Please type your estimate of the tax rate: %

SCREEN #136

Have you discussed this exercise with anyone who has previously been a participant?

Please type "N" to indicate no or "Y" to indicate yes:

SCREEN #137

THANK YOU

Thank you for participating in this study. To exit this program, press RETURN. Please return your materials to the proctor. He or she will explain how you can receive a written summary of the purpose and results of this exercise.

DEBRIEFING FORM

Thank You

Thank you for participating in this experiment. I will add your earnings to the earnings of others from your group. I will then forward a check to the leader of your organization.

Please do not communicate with anyone about this experiment for at least one month. This will help me insure that other participants will have no advantage when it comes to earning compensation.

Groups that send people to participate in this study can earn substantial amounts of money with little effort. If you know of any other community groups that may have an interest in participating in this study, please have them contact Ron Worsham at 336-4551.

If you are interested in receiving a brief summary of the purpose and results of this experiment, you may request this information from the researcher at the address below:

Mr. Ron Worsham
267 Business Bldg.
University of Florida
Gainesville, FL 32611

APPENDIX B
RESPONSES TO DEBRIEFING QUESTIONS

Question 1

Which of the following words best describes your feelings toward this exercise?

Type of Response	Number	Percentage
Very uninteresting	11	7.6%
Uninteresting	6	4.2%
Interesting	75	52.1%
Very interesting	52	36.1%

Question 2

Please choose the appropriate number where "1" means "very unrealistic" and "5" means "very realistic" as to how realistic you felt the simulation was:[50]

Type of Response	Number	Percentage
One was selected	7	4.9%
Two was selected	14	9.7%
Three was selected	43	29.9%
Four was selected	58	40.3%
Five was selected	22	15.3%
Mean		3.514
Standard Deviation		1.024

---

[50] All questions requiring numbers as responses required subjects to respond to a 5-point Likert-type scale.

Question 3

Please briefly describe what you think the experiment was about.

Type of Response	Number	Percentage
No response	10	6.9%
Did not know	42	29.2%
Tax reporting and investing behavior	71	49.3%
Tax reporting behavior only	19	13.2%
Effect of procedural injustice on tax reporting	2	1.4%

Question 4

Would you make the same taxpaying decisions in the real world that you made in this simulation?

Type of Response	Number	Percentage
Yes	111	77.1%
No	30	20.8%
No response	3	2.1%

Question 5

Please briefly describe how you would behave differently.

Type of Response	Number	Percentage
Would report more income on tax return	11	36.6%
Would change other behavior	18	60.0%
No response	1	.4%

Question 6

According to the instructions, was your audit rate on small business income higher than, lower than or the same as the audit rate on the small business income of the other group participating in this exercise?[51]

Type of Response	Number	Percentage
Higher than	40	56.3%
Lower than	18	25.4%
The same as	13	18.3%

Question 7

Please choose the appropriate number where "1" means "very unfair" and "5" means "very fair" as to how you would describe the difference between your audit rate on small business income and the same audit rate for the other group in this exercise:

Type of Response	Number	Percentage
One was selected	16	22.5%
Two was selected	10	14.1%
Three was selected	20	28.2%
Four was selected	10	14.1%
Five was selected	15	21.1%
Mean		2.971
Standard Deviation		1.434

---

[51] This question and the one below it were only presented to those subjects receiving the inconsistency treatment. Of the 72 subjects receiving this treatment, only 71 responded to the question. Accordingly, the percentages below were computed using 71 as the denominator.

Question 8

Was the letter you received at the end of the third year correct or incorrect concerning your failure to report your investment income?[52]

Type of Response	Number	Percentage
Correct	1	1.4%
Incorrect	69	98.6%

Question 9

Please choose the appropriate number where "1" means "very unfair" and "5" means "very fair" as to how you would describe having to pay additional taxes and penalties or audit costs as a result of receiving the letter:

Type of Response	Number	Percentage
One was selected	54	77.1%
Two was selected	5	7.1%
Three was selected	2	2.9%
Four was selected	4	5.7%
Five was selected	5	7.1%
Mean		1.586
Standard Deviation		1.234

---

[52] This question and the one below it were only presented to those subjects receiving the inaccuracy treatment. Of the 72 subjects receiving this treatment, only 70 responded to the question. Accordingly, the percentages below were computed using 70 as the denominator.

Question 10

Did all of the participants who have previously participated in this exercise earn as much as they should have?[53]

Type of Response	Number	Percentage
Yes	30	42.9%
No	40	57.1%

Question 11

Please choose the appropriate number where "1" means "very unfair" and "5" means "very fair" as to how you would describe how fairly the prior participants were compensated:

Type of Response	Number	Percentage
One was selected	6	8.6%
Two was selected	9	12.9%
Three was selected	24	34.3%
Four was selected	18	25.7%
Five was selected	13	18.6%
Mean		3.329
Standard Deviation		1.176

---

[53] This question and the one below it were only presented to those subjects receiving the indirect inequity treatment. Of the 72 subjects receiving this treatment, only 70 responded to the question. Accordingly, the percentages below were computed using 70 as the denominator.

Question 12

Have you discussed this exercise with anyone who has previously been a participant?

Type of Response	Number	Percentage
Yes	0	0.0%
No	141	97.9%
No response	3	2.1%

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I certify that I have read this study and that in my opinion it conforms to acceptable standards of scholarly presentation and is fully adequate, in scope and quality, as a dissertation for the degree of Doctor of Philosophy.

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April, 1994

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